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TESTS OF DISTRIBUTED ATTENTION: AN INVESTIGATION INTO THE DIFFERENCES BETWEEN TESTS WHICH APPEAR TO MEASURE CERTAIN ASPECTS OF DISTRIBUTED ATTENTION[1].

BY F. M. EARLE.

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I. INTRODUCTION.

WHEN the psychologist enquires of the workshop manager or foreman what are the chief abilities needed in the successful pursuit of some occupation, he is usually told that, among other things, the ability 'to pay attention' is very important. Sometimes such a statement is further qualified by terms implying 'concentration' and 'distribution.' It is supposed that in certain kinds of employment it is necessary to be able to 'concentrate,' whereas in other occupations it is essential to 'attend to several things at once' with accuracy and despatch. And it is often believed that clear-cut types of 'concentrators' and 'distributors' exist.

But according to Spearman, both the intensity and extensity of cognitive operations (*i.e.* 'concentration' and 'distribution' of attention) depend upon a general factor '*g*.' "In so far as they do so, the two constitute *alternative dimensions of the same cognitive output* characterising each individual." Further, he says, "neither the intensity nor the extensity generates any kind of group factor. Although faculties for each of these have been implied and stated very widely, and although upon them have been based projects and even actions of grave moment for education and industry, still, according to our present experimental evidence neither faculty has any real existence[1]."

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[1] An investigation carried out at the National Institute of Industrial Psychology with the aid of a grant from the Laura Spelman Rockefeller Memorial.

Even if the theory of psychological 'types'—such as 'concentrators' and 'distributors'—be no longer held, a practical problem still remains. The manager, the foreman, the storekeeper, the sales' assistant, the nurse, the machine-minder, the bus-driver are all called upon periodically to maintain continuity of action in a number of processes over a considerable period of time. What special characteristics does the 'attention to duty' of each of these persons possess, and in what ways do the qualities of attention needed in one occupation differ from those of another? Have, for instance, the intensity and the duration of attention necessary for success in teaching a class of children any connection with the qualities of attention required in 'minding' a complicated piece of machinery? Can the psychologist discover any individual differences in regard to 'attention' that may be of service in vocational guidance?

The problem is a difficult one because of the complications of other factors. Interest in the activity cannot be ignored as one factor in success; but we may, of course, assume that whenever interest in an activity *already* exists attention will be given at least with the intensity necessary for its successful completion. Even so, whenever some bodily action has to be performed, the efficiency of the performance is affected not only by the 'attention' given to the task but also by the bodily 'skill' of the performer. Correspondingly, in mental processes, there is the intellectual ability of the individual to consider; can we say, for instance, that the intensity of the attention of the 'duffer' at mathematics is one whit less than that of the 'intelligent' boy who finds the subject easy? It may be that in purely intellectual processes intensity of attention and ability are inseparable, but in physical activities this does not seem to be universally true. When I make a stroke at tennis the result depends more perhaps upon my muscular dexterity than upon my attention; for if I play carelessly (*i.e.* at less than my customary intensity of attention) it seems possible for my acquired neuro-muscular habits to complete the stroke with reasonable efficiency. But, of course, this could not go on indefinitely.

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[1] Spearman, *Abilities of Man.*

In any experimental work on these problems it seems desirable, therefore, to eliminate (as far as possible) complicating factors such as special knowledge, bodily dexterities and the like. The tests described below were devised with these aims in view. The results, while differing little in some respects from those of other investigators do, however, bring out interesting points which may prove useful in any further enquiries into these problems.

The tests were devised with particular reference to:

(*a*) The measurement of the intensity of the attention of the individual to a single object (which is not too complex) by the efficiency of the resulting cognitions.

(*b*) The measurement of the ability of the individual to maintain a succession of attentions to a number of objects, independently of any automatisms needed for success.

Most tests of attention hitherto used have followed the method of (*a*). Attention is measured at its maximal capacity by any convenient task in which output is easy to control and in which maximal output may be expected. But tasks such as the common ones of card-sorting, cancellation, etc., depend partly upon the manipulative dexterity of the individual, while the instantaneous exposures of the tachistoscope are not sufficiently like the practical procedures of everyday life to give much help in problems arising out of these. In the tachistoscope experiments, according to McQueen, the 'persisting image' explains success[1]; but although this plays a prominent part in everyday activities, in these the *duration* of exposure is much longer than in the tachistoscope. To measure attention satisfactorily we really need some activity which allows attention to express itself freely *without time limit*, even although speed of cognition be used as a partial measure of the efficiency of the attention. Provided that the object of attention does not require any special knowledge to interpret it, the speed as well as the accuracy of a particular cognition should give a fairly satisfactory measure of the intensity of attention by which it is gained.

In (*b*) we have an aspect of the problem which seems to have received less examination than it deserves. There are, of course, many forms of reaction-time apparatus which require a distribution of attention. In a multiple-choice reaction-time test, however, attention is usually directed by the unpractised subject more to the selection of appropriate response than to the stimulus itself. Certainly the stimulus, like the starting signal of a race, comes during a period of intense attention, but the distribution of attention varies greatly as between the stimulus and the response, and, as is well known, the speed of reaction varies accordingly. In a more practical activity such as driving a motor car in which similar conditions prevail, although attention must also be distributed between the various stimuli and the appropriate responses, there is seldom need for the highest speed of response. The reactions required are hardly ever so sudden that they need to be measured in thousandths of a second. Each response as it occurs comes after a period of preparation (long or short according to circumstances)—there is always an anticipation that, in a moment or so, this or that movement of lever or wheel will have to be made. Moreover, in the skilled driver the purely manipulative processes are purely automatic; brake levers do not require searching for. Hence, given automaticity in the operation of hands and feet, the efficiency of the motor driver's performance depends almost wholly upon the clearness with which sense impressions are received and interpreted. Even in emergency, when accident is avoided by a hair's breadth, this remains true; the best driver is not one who overlooks (deliberately or otherwise) the significance of the events leading up to the 'tight corner,' although he may succeed in getting out of it by quick thinking and rapid action. Generally the quick response of the good driver is an indication of his 'preparedness.' In practical situations when inefficiency or 'flurry' occurs, an explanation may be found in the failure of perception; important signs will be overlooked or their significance perceived too slowly, and the result is a congestion of mental processes and an imperfect response. This slowness of perception may be due to the inability of the individual to attend to several things quickly enough, to a lack of experience preventing him from understanding the significance of a particular sense impression (the failure of the novice), or to emotional excitement. The importance of securing an equality of experience and knowledge in the processes to be used as a test of attention seems obvious. Emotional excitement and failure of perception may, however, be closely associated; for the latter may be one of the ways in which a tendency to 'excitement' under the most ordinary circumstances expresses itself.

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[1] McQueen, "The measurement of attention," *Brit. J. of Psychol.* Mon. Supp. 1917, v.

Hence the first of the following tests was designed to reproduce conditions in which attention could be given at high intensity over a narrow range, the second to provide a situation in which attention could be given successively (*i.e.* distributed) to a number of objects within a comparatively short time[1]. In both cases the objects were to be apprehended visually, this being the form of perception usually employed in everyday activities. The manipulative elements were reduced to a minimum and there was little or no need for any previous experience or knowledge.

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[1] Short as compared with the duration of ordinary activities, long as compared with the exposures of the tachistoscope.

II. Description of tests used in this enquiry.

Test I. A test of speed and accuracy of perception.

The apparatus used in this test was as in Fig. 1. It consists of a large wooden disc 12 in. in diameter and ½ in. thick, enclosed in a shallow box in such a way that it rotates easily about a central axis. The disc is so arranged that part of it projects sufficiently beyond the edge of the box to enable the experimenter to move it easily and quickly. In the cover of the box a rectangular hole 2 in. × 1 in. is cut in such a position that, as the disc is rotated, the diagrams pasted on its surface may be successively brought into view.

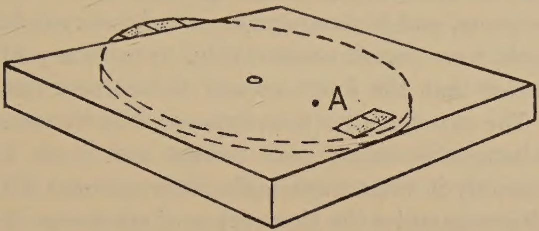


Fig. 1.

The standard diagrams were like this:

XVIII		XVIII
Poor		Cool
θθθθθθθθ	*or*	θθθθθθθθ
θθθθθθ		θθθθθθ
θθθθ		θθθθ
θθ		θθ
food		moon

It will be observed that the elements consist of:

(i) A Roman numeral.

(ii) An adjective and a noun forming the phrases 'poor food,' 'cool moon.'

(iii) A symmetrical arrangement of 'modified' O's (obtained by superposing two typewriter letters such as O and o; O and e, etc.).

The two diagrams shown above were pasted on the outside of the box (at A) near the aperture at which the diagrams on the disc were successively exposed. Forty similar diagrams were pasted on the disc so that they were clearly visible at the window as the disc was rotated. Half were like the models; the incorrect diagrams were formed by altering:

(*a*) The shape (but never the symmetrical arrangement) of the O's; thus ϴ ⊖.

(*b*) The combination of adjective and noun in such ways as 'cool food,' 'fool moon,' etc.

(*c*) The Roman numerals.

In a few of the incorrect diagrams all three errors were introduced, in others two errors, and in some one error only was made.

The subjects were seated comfortably, in such a position opposite the experimenter that the aperture and the correct models could be clearly seen. The experimenter then exposed, one by one, six trial diagrams, explaining that some were correct and some incorrect, and pointing out exactly in what respects the incorrect ones differed from the models. In these examples the three types of error were illustrated, and it was explained to the subject that he would be required to pick out similar errors in other examples. The subject was then asked if he understood clearly what he had to do and, as a precaution, the six diagrams were again successively exposed so that the subject could respond to them in the proper manner. Forty diagrams were then successively exposed. The responses of the subject were noted in detail, showing how many of the elements were correctly observed. The experimenter himself operated the disc so that the subject had only to watch the diagrams and make the appropriate responses. The subjects were asked to point to the errors, and not merely to mention their number. They were also advised to look carefully before responding, but to do so as quickly as possible. Hence, as soon as the subject gave his judgment, the experimenter passed on to the next diagram, and then noted the result of the previous exposure during the time the subject was examining the new diagram. At the end of the series the total time spent by the subject on the task and his total errors were recorded.

The detailed instructions for giving the test are as follows:

Show the subject the apparatus and demonstrate how the disc may be operated.

"Here is a box with a wheel inside it which can be turned round like this. [Demonstrate.] On the wheel there are a lot of little drawings on which there are also words and numbers. By turning the wheel these drawings keep appearing at the little window here. [Point.] Do you see?

"Now here is the drawing you have to look out for. [Point to sample pasted on the board.] Notice how it is arranged: notice also the words. Both sets of words are right—both 'poor food' and 'cool moon'; that's why we have put two samples for you to look at.

"Now here [point to first diagram of the series now exhibited] is a picture which is just like the sample one. You see that in every detail it is like the one you have to look for. Now I turn the wheel round. Is the next one a correct copy? Yes, it is a correct copy. The two diagrams are alike. And the next? No, that is not a correct copy. [Point out errors.] And the next? Again it is not a correct one. [Point out errors.] And the next? Yes, that is correct. And the next? Once again the diagram is incorrect. [Point out errors.]

"Now you see how it works. Some of the diagrams are right and some of them are wrong. Well, what I want you to do, is to watch as I turn the wheel round and say 'correct' if the diagram is quite correct but point out the mistakes if there is anything wrong. Do you understand?

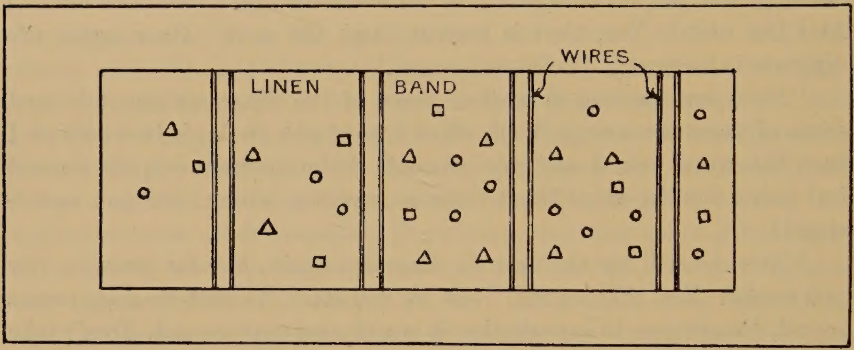
"Now we will try the first six diagrams again, just for practice. Are you ready? Yes, that's good. Now we will start. As each diagram comes round, I want you to say whether it is a correct copy or not. Don't take too long over each one. Just satisfy yourself which it is as quickly as possible and then tell me."

The kind of attention required in this test is exemplified in the 'inspecting' processes in the factory. The worker passes or rejects the finished product according as it does or does not equal the standard. This process is similar to that of searching (as in the cancellation test) for 'o's' or 'e's' in the lines of a printed page, except that the 'standard' is more complex, and several points may have to be looked for at one inspection. Consequently a fairly complex object must be used in this test, so that both coarse and fine errors can be introduced. Moreover, the manipulative element (as it enters in cancellation and similar tests) must be excluded. Success in the task, so far as it can be analysed, depends upon the intensity of attention during the exposures, and the accuracy and speed of perception resulting. Memory comes in only slightly because the models are constantly in view.

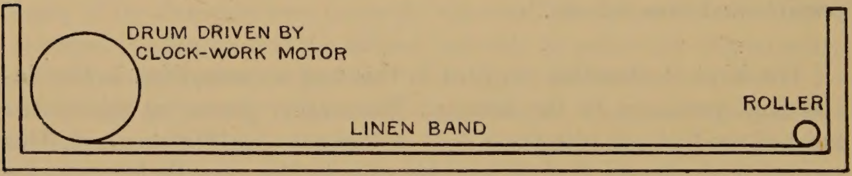
Since there are at least three items (or parts of the diagram) to be examined, it might appear that a distribution of attention also enters into the process. Certainly a rapid survey of the whole diagram is needed. But discussion of this point may conveniently be deferred until we have described the second test.

Test II. A test of distribution of attention over objects in a visual field large enough to require frequent and considerable shiftings of the eye's fixation point.

The apparatus (see Fig. 2) consists of a wooden screen with a window in it of dimensions 24 in. × 6 in. A long linen band 6 in. wide is made to travel slowly from right to left behind this window. On it are a number of coloured geometrical figures which appear in view on the right and eventually pass out of sight at the left. The rate of movement is comparatively slow, so that it is possible for the subject to traverse by eye the whole of the linen band exposed during the time that the figures are moving through only a short distance. Four pairs of black wires are stretched across the window at equal distances apart.



Front view



Plan

Fig. 2.

The space enclosed between each pair of wires is sufficient to allow one geometrical figure at a time to appear wholly within it. The figures used are squares, circles and triangles, of approximately equal area, and coloured green, yellow and red, singly and in various two-colour combinations. The figures which the subject is required specially to observe are respectively green triangles, red circles, and yellow squares. Yellow triangles, red squares, etc., are ignored. When any one of the selected three figures appears within any pair of wires a response has to be made. This response is of as simple a kind as possible. Plain cards are given to the subject, and he is asked to put a card in one of three boxes according to the figure to which he is responding. For example, the response to a red circle consists in placing a card in the red circle box, which is plainly marked by a large drawing of a red circle fixed above it. (It would, however, be better to employ three keys to register these responses.)

The general instructions for giving the test are as follows: The examinee is to be seated comfortably at the table on which the apparatus is placed. He should be at least 2 ft. (if possible 3 ft.) from it. The response cards are arranged in a pile on the table in front of the subject. He may take some in his hand if he wishes to do so. The three boxes in which these cards have to be put are placed directly in front of the apparatus and about 6 in. apart.

The general nature and purpose of the apparatus (including the mechanism—as a matter of interest and to avoid distraction due to curiosity as to 'how it works') are explained to the subject, and the motor is then started. The first two signals are responded to by the examiner, cards being placed in the appropriate boxes, in the exact manner in which the subject is required to respond. He is then asked if he clearly understands what he has to do. This being the case he is allowed to make the responses to the next signals in order to become accustomed to the rate of movement. The motor is again stopped and the subject is asked if he is ready, and then the command to begin is given when the motor is started for the third time.

The preliminary conversation usually takes this form:

"You see this set of drawings here. They are drawn on a sheet of linen which I have arranged in such a way that I can make it move slowly. [Show back of apparatus.] When I start the clock-work motor here, it wraps the linen sheet round this roller. Do you see? Now take a seat here so that you can see all the drawings. Is that comfortable? Now I want you to notice these black wires. There are four sets of them. [Point them out in turn.] Now as the linen band moves, the diagrams on it pass behind these wires, just as this one is doing now [point one out], so that at certain times a drawing will be exactly between one pair of black wires. Do you understand that?

"Now there are a great many drawings on the sheet but there are only three kinds that matter. You see them here [point to models on boxes]—a yellow square, a red circle and a green triangle. Do you see them? Say what they are? [A yellow square, etc.] And here are some plain cards. What I want you to do is to watch the linen band as it moves, and whenever one of these three drawings [point to them again and name them] comes right between any pair of wires you must pick up one of these cards [point] and put it in one of the boxes which you see here [point]. It must go into the box, the drawing above which corresponds in shape and colour with the drawing which can be seen between one of the pairs of wires. If a green triangle appears between any of the wires, then you put a card into the green triangle box. Do you understand? Watch me do the first two and then you can have a try. [Start motor and demonstrate with first two signals.] Now you try, this is only for practice. [Practice on next three signals.]"

The general instruction to respond only to the red circle, green triangle and yellow square is repeated, and then the experiment proper is begun. All errors are recorded.

The order of signals was so arranged that a response was required after an interval varying from 3 seconds to 17 seconds. The average interval was approximately 7 seconds.

Visual presentations were used in this test as being most easily arranged and most commonly employed in practical situations. The aim was to present within the field of vision a number of objects moving simultaneously across the field. In ordinary circumstances attention can either be directed to the movement of one object during the whole of its journey, or it may be given successively to different objects. In this test, however, the latter type of attention, *i.e.* successive attention to a number of objects, is compulsory, since no single fixation point will permit apprehension of the whole field.

The abilities required for success in this task appear to be:

(*a*) To remember the 'adequate' stimulus—red circle, green triangle and yellow square.

(*b*) To remember that different responses are required to each and to select the correct one. (But each signal suggests its own response.)

(*c*) To distribute attention over all the four pairs of wires (several subjects temporarily forgot one of the pairs).

(*d*) To establish a succession of attentions between the various competing stimuli.

Of these (*c*) and (*d*) appear to be the most difficult. Sooner or later the various irrelevant stimuli (such as all the other coloured drawings) must cease to receive any conscious attention; in other words, the intensity of attention for the three significant drawings must be sufficiently high to exclude all the others, but not so high that attention to one of the three significant drawings tends to prevent attention to the other two. This equilibrium of attention having been secured for the three drawings, they have then to be located as 'approaching,' 'half within,' or 'wholly within' one of the pairs of parallel wires. Suppose that one response has been made—there is now time to traverse the field visually to locate the position of the drawing which requires the next response (many subjects can go further and locate a succession of such drawings). Hence the next stage of the process, viz. the discriminative reaction, does not come without warning; there is usually sufficient time to locate the proper response box (in fact many subjects show their anticipation by selecting the appropriate response and holding themselves in readiness to carry it out the moment the signal arrives). To that extent, therefore, a subject's quickness of reaction to a given signal does not, within certain limits, influence the results. On the other hand, when a subject overlooks a signal and only notices it at the last moment, his response is apt to be flurried and this may affect him for several successive responses. But the time allowance appeared to be sufficient to allow the great majority to recover quickly even after the belated discovery of an overlooked signal. In this connection it is to be borne in mind that the test seeks to pick out those who get 'flurried' under comparatively simple conditions. It seems reasonable to suppose that such subjects would respond equally badly in more complex situations.

Differences between Tests I and II.

It would appear on analysis that the two tests differ in this respect:

Test I requires a high intensity of attention over a narrow range of objects (three or four elements in the whole).

Test II requires a lower intensity of attention over a wider range of objects (three objects, four positions, three reactions).

As regards the manipulative aspects of Test II, the reaction is so simple that it may be assumed to be 'automatic' almost from the start. This is true of cancellation tests also, but the relation between the time required to perform each act and the test as a whole is of some importance. In this test the proportion of time spent in actual manipulation is small. Attention is directed to locating the proper box for response rather than to picking up a card (because this is such a very common process and everyone can do it 'sub-consciously' or 'marginally'). Further, success in Test II does not depend on memory (*e.g.* remembering the kind of response), because each fresh signal for a response gives its own cue. In other words, the discriminative reaction is of the very simplest type and does not require the memorizing of a series of alternative situations and responses. Thus the verbal accompaniment (sub-vocal) of a response when it occurs might be expressed something like this. "Here's a red circle coming" (he picks up a card). "Where's that red circle box? There it is [moves hand to proper box]." "There it goes [drops card]." "Now where's the next one? There goes a triangle. No it isn't (colour is wrong, perhaps)." "Ah! Here's a yellow square!" [In many cases this attention to the next signal will take place as soon as the automatic process of dropping the card and picking up another one has been started on its way; in some cases it may even begin *before* this physical activity is allowed to begin.]

In the case of Test I, however, it is possible that memory for a complex diagram visually presented may be a contributing factor (cf. McQueen's 'persisting image'). Although the correct models are constantly in view (for purposes of reference when necessary), it was observable that many subjects do not refer to them at all after the correct form has been learnt. It was thought desirable, therefore, to compare the results of this experiment with those obtained from a test in which memory was more directly employed. For this purpose two supplementary tests were used. They were:

Test III. A test of perception of form in which immediate memory enters.

Forty different rectilinear diagrams were prepared resembling that given in the sketch (Fig. 3). Variations in them were made by changing the relative size of the sections, and by changing the positions of the diagonal lines and the cross. The diagrams were arranged in pairs so that some of the pairs consisted of two identical diagrams, while in others the diagrams were unlike in some way.

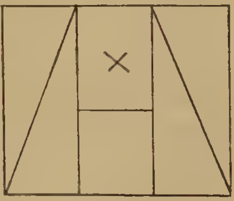


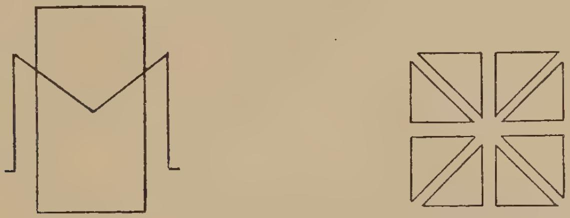
Fig. 3.

The procedure was as follows. One of the diagrams of a pair was exhibited for 10 seconds. It was then covered over, and the other diagram of the pair was similarly exhibited. The subject was required to say whether the drawings were exactly alike or, if not, in what respects they differed. (Suitable explanations and practice trials were first given.)

Such a test requires the clear perception of detail, whether or not the object is consciously analysed and noted. Unless this perception of the first diagram is accurate and clear, the exposure of a second diagram (which differs only slightly from the first one) will tend to produce uncertainty and doubt. (This uncertainty may also occur when the diagrams are identical, especially if the recall be at all imperfect; but in material of this kind it may be less difficult to recognize identity than difference.) It was observed, however, that a large proportion of the subjects examined were able to specify the points of difference with considerable accuracy.

Test IV. Another test of perception of form in which immediate memory enters.

In this test eight rectilinear diagrams of gradually increasing complexity (see Fig. 4) were exposed one by one for 15 seconds. After each exposure the subject was asked to sketch the diagram he had just seen in as correct shape and proportion as he could. Marks were awarded for correctness in reproducing the symmetry, proportions and details of the drawings, according to a fixed scale.



First diagram. Eighth diagram.

Fig. 4.

Confusion arises in this test chiefly owing to the vagueness of the images which are recalled. Since there is a symmetrical arrangement in nearly every drawing which an intelligent observer usually notes, and which serves as an aid to recall, it is to be expected that the results of the test will disclose a connection with what is commonly called 'general intelligence.' In other words, successful recall in this case seems to depend upon the 'organization' as well as upon the clearness and intensity of the perceptions involved.

Tests V–IX.

But immediate memory for a visual impression is not the only special factor which requires consideration in these tests. Consequently it was considered useful to compare the results with those obtained from the following additional tests.

Test V. The Stanford Revision of the Binet Intelligence tests as revised for London children.

Test VI. A Group Intelligence test, No. 34, devised by the National Institute of Industrial Psychology for use with elementary school children of age 13–14.

Test VII. A test of Substitution (Woodworth and Wells)[1].

Test VIII. A Maze test (Porteous)[1].

Test IX. A Group test of Form Relations[2].

The importance of making a comparison between these tests and Tests I and II is apparent. We are concerned in this enquiry with the form of objects. It may, therefore, be helpful to observe how successful the subjects are in dealing with simple problems in which only the ability to perceive quickly and accurately relations of shape and form is required. In a test such as Test IX differences in capacity to remember do not enter at all, the forms and their parts being observable on the same page throughout the whole time allowed for the solution of the problem. Similarly, since the organization of experience, immediate or remote, contributes to the successful performance of the various tasks employed in the enquiry, some measure of intelligence is necessary. We were fortunate enough to be able to obtain measures of general intelligence from both group and individuals tests, and from both the so-called 'verbal' and 'non-verbal' types of test also.

The possible effects of imperfect vision upon the processes studies in these tests were also considered. As will be seen (p. 230) there were no cases of visual defect which seemed sufficiently serious to affect the general results.

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[1] Tests selected from the Scale of Performance tests standardized by F. Gaw for London School children. (*I.F.R.B. Report*, No. 31.)

[2] The Form Relations test consists of a number of geometrical figures so drawn that in each one a portion of the figure is shown to be missing. The subject is asked to choose from among a number of alternatives the 'part' which belongs to each incomplete 'whole.' The figures are arranged in sets of five, the sets being graded in difficulty and presented separately.

III. STUDY OF RESULTS OBTAINED.

All the children were of age 13 years 9 months, 13 years 10 months, 13 years 11 months. There were 37 boys and 41 girls. They were of elementary school education and training and were in their last term of school life. They were of working class parentage.

(1) *Sex differences.*

The following table shows the mean and median scores for boys and girls, with the differences between them and the probable errors of these differences:

Table I.

Test	Sex	No. of cases	Mean	Median	Sex difference between means	Probable error
I	Boys	37	10·76	11·0	1·35	±0·89
	Girls	41	9·41	9·5		
II	Boys	37	12·75	13·0	0·57	±0·7
	Girls	41	13·32	13·5		
III	Boys	37	12·24	12·0	0·16	±0·47
	Girls	41	11·64	12·0		

The scores are given as the number of 'errors' made. Maximum error score is 40.

On these figures the differences between the mean scores for boys and girls (which are in Tests I and III opposite in sign to those in Test II) are no larger than might be expected by purely chance circumstances. For this reason, whenever it was desired to compare the results of these three tests with others in which, similarly, no sex differences were observable, the above groups were combined into a single group of 78 cases.

The median and quartile scores for the combined group are given in Table II.

Table II. *Median and quartile scores for Tests I, II and III* (78 *cases*).

Test	10th percentile	25th percentile	Median	75th percentile	90th percentile	Mean
I	17	14	10	6	3	10·0
II	20	16·5	13	9·5	6·5	13·05
III	9	10	12	14·5	16·5	11·9

(2) *Relation of test results to visual acuity.*

The scores in Tests I and II were considered in relation to the quality of vision possessed by the subjects. The following results do not suggest that visual acuity produced any important effects. The ratings for vision were given by the visiting medical officer[1].

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[1] In connection with an experiment in vocational guidance carried out by the N.I.I.P.

Table III.

	Test I		Test II	
Visual acuity	Score range	Median	Score range	Median
$\frac{6}{6}$	0–23	10	3–22	12
From $\frac{6}{6}$ to $\frac{6}{12}$	2–21	12	2–25	16
From $\frac{6}{12}$ to $\frac{6}{18}$	2–16	7	7–23	15
Worse than $\frac{6}{18}$	0–17	10	9–24	14

There is clearly no direct correspondence between errors and quality of vision.

(3) *Relation between time taken and errors made in Test I.*

In Test I it was arranged that the subject should be allowed as much time as he required for the inspection of each diagram (though the general instructions emphasized that the inspection should be done quickly). The question arises whether the slower and more deliberate observers are more accurate on the whole than the quicker ones. The view that the steady plodder never does himself complete justice in a test in which there is a time limit is constantly advanced by those who are disposed to question the validity of intelligence and other tests. In the present case the results show that, so far as the abilities required for success in this test are concerned, there is no ground for this belief.

	Boys (37)	Girls (41)	Whole group (78)
Mean time taken to complete the inspection of 40 diagrams	266·4 sec.	*289·1 sec.	277·1 sec.
Standard deviation	72·3 sec.	104·9 sec.	90·5 sec.
Correlation with score ...	$-0{\cdot}38 \pm 0{\cdot}09$	$-0{\cdot}44 \pm 0{\cdot}085$	$-0{\cdot}37 \pm 0{\cdot}066$

* Sex difference between means $-22{\cdot}7$ sec. P.E. of difference $\pm 13{\cdot}6$.

Although the correlation between the time taken and the total score is not large, it is significant and it indicates that there is a definite tendency for the more rapid observers to make the fewer mistakes.

(4) *Comparison between the results of different tests.*

In Table IV the inter-correlations between tests are given for boys and girls combined in those tests in which no sex differences appear. In Tables V and VI the sexes have been treated separately.

Table IV.

Test	No.	I	II	III	IV	V	VI	VII	VIII
Inspecting	I	—	·29	·41	·31	·41	·37	·30	·26
Machine-minding ...	II	·29	—	·24	·35	·26	·25	·13	·14
Pairing	III	·41	·24	—	·22	·27	·22	·27	·32
Memory drawing ...	IV	·31	·35	·22	—	·61	·58	·45	·44
Intelligence (Binet)	V	·41	·26	·27	·61	—	·81	·53	·47
Intelligence (Group)	VI	·37	·25	·22	·58	·81	—	·48	·41
Substitution ...	VII	·30	·13	·27	·45	·53	·48	—	·42
Maze (Porteous) ...	VIII	·26	·14	·32	·44	·47	·41	·42	—

Table V. *Inter-correlations between tests* (37 *boys*).

Test	No.	I	II	III	IV	V	VI	IX
Inspecting	I	—	·33	·35	·19	·33	·28	·42
Machine-minding ...	II	·33	—	·03	·26	·08	·14	·21
Pairing	III	·35	·03	—	·17	·30	·14	·05
Memory drawing ...	IV	·19	·26	·17	—	·46	·41	·32
Intelligence (Binet)	V	·33	·08	·30	·46	—	·81	·32
Intelligence (Group)	VI	·28	·14	·14	·41	·81	—	·19
Form Relations ...	IX	·42	·21	·05	·32	·32	·19	—

Table VI. *Inter-correlations between tests* (41 *girls*).

Test	No.	I	II	III	IV	V	VI	IX
Inspecting	I	—	·25	·42	·43	·24	·46	·14
Machine-minding ...	II	·25	—	·42	·32	·26	·25	·37
Pairing	III	·42	·42	—	·32	·19	·21	·12
Memory drawing ...	IV	·43	·32	·32	—	·72	·63	·53
Intelligence (Binet)	V	·24	·26	·19	·72	—	·86	·47
Intelligence (Group)	VI	·46	·25	·21	·63	·86	—	·69
Form Relations ...	IX	·14	·37	·12	·53	·47	·69	—

From these tables it would appear that the relations between the various tests are as follows:

(1) Test I correlates best with intelligence for girls, and with form relations for boys.

(2) Test II correlates best with Test III for girls and Test I for boys, but in both cases correlation with immediate visual memory as measured by Test IV is significant. This is not surprising when the processes in the two tests are compared.

(3) Test III correlates best with Test I for both girls and boys. This suggests that the inspection of given forms has a special characteristic of its own.

(4) The average correlations with Tests V, VI, VII and VIII for the combined group is:

Test I, 0·335; Test II, 0·19; Test III, 0·27.

The average correlation with Tests IV, V, VI, VII and VIII for the combined group is:

Test I, 0·33; Test II, 0·22; Test III, 0·26.

If these additional tests be regarded as measures of intelligence (separately or combined) the correlation of Tests I, II and III with intelligence is not high.

Other methods of examining these tables of coefficients are, however, available. We may apply the partial correlation formulae of Yule to see if any significant relationships remain when the factors measured by one kind of test are considered as without influence on the results of other tests.

In Table VII the partial correlations obtained from the application of Yule's formulae to selected coefficients of Table IV are given. The variables selected are: (1) Memory drawing (Test IV); (2) Intelligence (Test VI); (3) Inspecting (Test I); (4) Machine-minding (Test II). Thus the coefficient $r_{34\cdot12}$ (0·058) signifies the extent of the relationship between 'inspecting' and 'machine minding' when the effects of 'intelligence' and 'immediate visual memory' are held constant.

Table VII.

$r_{12}=\cdot58$	$r_{12\cdot3}=\cdot56$ $r_{12\cdot4}=\cdot54$	$r_{12\cdot34}=\quad\cdot525$	Correlation (due to '*g*' ?) unaltered by the two 'attention tests'
$r_{13}=\cdot37$	$r_{13\cdot2}=\cdot25$ $r_{13\cdot4}=\cdot31$	$r_{13\cdot24}=\quad\cdot248$	
$r_{14}=\cdot25$	$r_{14\cdot2}=\cdot023$ $r_{14\cdot3}=\cdot26$	$r_{14\cdot23}=-\cdot026$	
$r_{23}=\cdot31$	$r_{23\cdot1}=\cdot234$ $r_{23\cdot4}=\cdot127$	$r_{23\cdot14}=\quad\cdot081$	
$r_{24}=\cdot35$	$r_{24\cdot1}=\cdot132$ $r_{24\cdot3}=\cdot286$	$r_{24\cdot13}=-\cdot16$	Correlation between 'machine-minding' and 'intelligence' (due to '*g*' ?) removed when visual memory test (which possesses a high '*g*' factor) is held constant.
$r_{34}=\cdot29$	$r_{34\cdot1}=\cdot224$ $r_{34\cdot2}=\cdot205$	$r_{34\cdot12}=\quad\cdot058$	Correlation between 'inspecting' and 'machine-minding,' reduced when effect of 'intelligence' is held constant

Table VIII. *Partial correlations for the four variables.*

(1) Intelligence (Stanford Binet); (2) Inspecting;
(3) Machine-minding; (4) Pairing.

$r_{12}=\cdot41$	$r_{12\cdot3}=\cdot36$ $r_{12\cdot4}=\cdot34$	$r_{12\cdot34}=\cdot33$
$r_{13}=\cdot26$	$r_{13\cdot2}=\cdot16$ $r_{13\cdot4}=\cdot21$	$r_{13\cdot24}=\cdot13$
$r_{14}=\cdot27$	$r_{14\cdot2}=\cdot12$ $r_{14\cdot3}=\cdot22$	$r_{14\cdot23}=\cdot08$
$r_{23}=\cdot29$	$r_{23\cdot1}=\cdot22$ $r_{23\cdot4}=\cdot22$	$r_{23\cdot14}=\cdot14$
$r_{24}=\cdot41$	$r_{24\cdot1}=\cdot34$ $r_{24\cdot3}=\cdot32$	$r_{24\cdot13}=\cdot13$
$r_{34}=\cdot24$	$r_{34\cdot1}=\cdot18$ $r_{34\cdot2}=\cdot19$	$r_{34\cdot12}=\cdot09$

Study of Table VII suggests that the correlation between Tests I and II and the others (as well as between themselves) is due to a factor which is found to a high degree in the Intelligence Group test and in the Immediate Visual Memory tests. In Table VIII we have a similar result.

The same method may be applied to the remainder of the variables in Table IV but for our immediate purposes it is unnecessary.

In some respects a more convenient method of analysis is provided by the use of Spearman's tetrad-difference criterion.

If the correlations in Table IV be examined no very large tetrad differences are obtainable. The following are some of the largest:

Table IX.

	Intelligence (Group)	Machine-minding	
Stanford Binet	·81	·26	$F = \cdot81 \times \cdot35 - \cdot58 \times \cdot26$ $= \cdot2835 - \cdot1451$
Memory drawing	·58	·35	$= \cdot1384$
	Memory drawing	Machine-minding	
Stanford Binet	·61	·26	$F = \cdot81 \times \cdot29 - \cdot31 \times \cdot26$ $= \cdot1769 - \cdot0806$
Memory drawing	·31	·29	$= \cdot0963$
	Intelligence (Group)	Machine-minding	
Stanford Binet	·81	·26	$F = \cdot81 \times \cdot29 - \cdot37 \times \cdot26$ $= \cdot2349 - \cdot0962$
Inspecting	·37	·29	$= \cdot1387$

The P.E. of $F = 0{\cdot}037$[1] (five times the P.E. of $F = 0{\cdot}185$). There are, therefore, no tetrad differences greater than five times the P.E.

Hence, according to Spearman's theorem, the correlations between these eight tests are entirely due to the operation of a common factor '*g*,' and the correlation between each test and '*g*' may be calculated. These correlations[2] are as follows:

Table X.

	Correlation with '*g*'
Intelligence (Stanford Binet)	·83
Intelligence (Group) ...	·76
Memory drawing	·71
Substitution	·61
Mazes (Porteous)	·57
Inspecting	·55
Pairing geometrical forms ...	·44
Machine-minding	·37

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[1] Formula in appendix, Spearman's *Abilities of Man.*

[2] Calculated from the formula $r_{ag} = \sqrt{\dfrac{A^2 - A'}{T - 2A}}$.

It is apparent that neither Test I nor Test II is as good a measure of '*g*' as the so-called 'intelligence' tests, but that to a certain extent '*g*' enters into success in each. There are, however, large specific factors to be considered. According to Spearman, since the tetrad difference criterion is satisfied, none of these specific factors should cause any significant amount of correlation when the influence of '*g*' is removed.

Using Yule's formula for partial correlations the inter-correlations of the eight tests for '*g*' constant may be calculated. They are given in Table XI.

Table XI. *Inter-correlations for* 'g' *constant* (*derived from Table IV*).

(Arranged in order of Table X.)

Test	No.	V	VI	IV	VII	VIII	I	III	II
Intelligence (Binet)	V	—	·49	·05	·07	−·03	−·09	−·19	−·09
Intelligence (Group)	VI	·49	—	·04	·04	−·05	−·08	−·20	−·05
Memory drawing ...	IV	·05	·04	—	·04	·06	−·13	−·15	·13
Substitution ...	VII	·07	·04	·04	—	·11	−·05	−·10	−·13
Mazes (Porteous) ...	VIII	−·03	−·05	·06	·11	—	−·08	·09	−·09
Inspecting	I	−·09	−·09	−·13	−·05	−·08	—	·23	·11
Pairing	III	−·19	−·20	−·15	·01	·09	·23	—	·09
Machine-minding ...	II	−·09	−·05	·13	−·13	−·09	·11	−·09	—

There are only two coefficients in this table that are large enough to require comment. Theoretically all of them should be of zero value, so that the question arises whether any of them are too large to be explained by the unavoidable errors of measurement.

The two exceptional coefficients are those between the two Intelligence Tests V and VI (0·49) and between Test I and Test III (0·23). In both these cases there is an important resemblance between the kind of material employed and the mental processes which take place. Inasmuch as 'overlap' may occur between tests which resemble one another[1], we seem to have here two examples of this phenomenon.

Similar results have been obtained for a different correlation table made up of eight variables, the test of Form Relations (Test IX) being, in this case, included in place of the Substitution test. The distribution of tetrad differences is now as in Fig. 5.

It will be observed that the tetrad difference criterion is once again satisfied. Moreover a specific correlation (0·46) remains between the Stanford Binet test and the Intelligence Group tests (and also between Test I and Test III) when the effect of '*g*' is held constant, but in this case there also appears to be an 'overlap' between the Memory Drawing test and the Form Relations test (represented by a coefficient of 0·22).

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[1] Cf. Spearman, *op. cit.*

We may conclude, therefore, that although the correlations in Table IV are largely due to the influence of a common factor '*g*,' there is some similarity between Tests I and III which produces an additional correlation between them. For the rest there are no other connections. The results of each test are influenced by large specific factors, and we may regard success in Test II as being mainly due to such specific factors, the effects of '*g*' being in this case small.

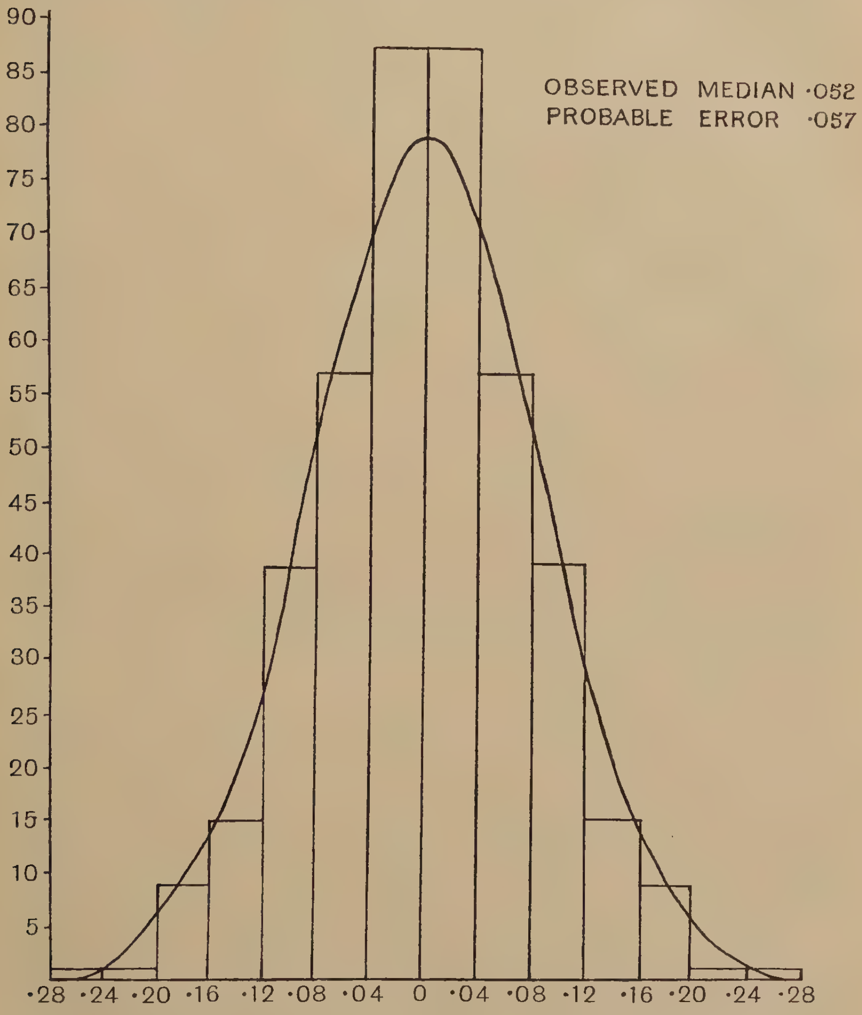

Fig. 5. Observed and theoretical distribution of tetrad differences.

IV. CRITICAL REVIEW AND CONCLUSIONS.

It is clear from the results of the experiments described in the last section, as well as from the work of previous investigators, that the problem of measuring differences in the ability of persons to 'pay attention' is a difficult one. It is, apparently, impossible to avoid the complications of other factors, and although we may reduce the effects of experience or of acquired manual dexterities, as we have done in these enquiries, we cannot remove the influence of general intelligence or whatever that general ability may be, which finds expression in all mental activities.

It has been suggested that the so-called general intelligence is in fact the ability to 'pay attention.' But it has been called other things too, such as 'creative ability' and 'reasoning power,' and while it may be true that the quality of attention is a factor in intelligence behaviour, it hardly seems likely that intelligence and the quality of attention are identical. McQueen[1] in his researches concluded that there was no 'power,' either to distribute attention or to concentrate attention, of general application and capable of measurement, and he was inclined to regard the connections which he found between dissimilar tests as the effects of the general factor '*g*.'

Spearman[2] has endorsed this view and has declared that attention in its different aspects merely represents 'alternative dimensions of the same cognitive output characterizing each individual.' This is as much as to say that there is no ability 'to attend to several things at once' which is distinctly different from the ability 'to concentrate attention upon a single object.' The capacity for success in activities dependent upon these is due, according to Spearman, to a general ability which expresses itself in either way, while any abilities which are called out by one task rather than another are regarded as special and peculiar to that particular task. Consequently there can be no 'group' factors of concentration or of diffusion of attention (additional to the general ability above mentioned), which may be shown in several dissimilar activities.

But in all the tests mentioned by Spearman[3] in his argument on this particular subject, attention, as given, appears to consist of a succession of concentrations, the diffusion of which he speaks being chiefly shown in the continuity of purpose. Moreover the processes required in those tests are of a highly intellectual order, and possibly are not suitable for comparison with those in which attention may be said to be given at lower perceptual levels. Many of the processes of everyday life which are spoken of as requiring a distribution of attention are of the latter kind. Mental and manual processes often go together. If we consider the 'non-mental' aspects of manual dexterity, and the fact that tests of dexterity usually correlate very slightly with tests of intelligence, it is difficult to see how the conditions of attention in these are related to the conditions of attention in more intellectual processes, simply through the operation of a general factor which chiefly appears in the latter.

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[1] McQueen, *op. cit.*

[2] Spearman, *op. cit.*

[3] Spearman, *op. cit.*

Be that as it may, there can be no doubt as to the generality of some factor underlying success in all the tests described in this report. The results obtained are all consistent with the belief that success in these activities is dependent, in part at least, upon a general factor (Spearman's '*g*'). But the correlation between the tests specially introduced as measures of 'attention' and those in which the '*g*' factor is apparently dominant is not as high as might be expected if success in them were *largely* due to the influence of this factor. Hence we can only conclude that in each of these two tests specific factors are operative to a considerable degree. (By 'specific' is meant peculiar to each test or process.) Apart from a suggestion of 'overlap' between Tests I and III (and of course between the two verbal intelligence tests), there is no suggestion of any relationship between Tests I and II or between any other pair, other than that due, on Spearman's theory, to the general factor '*g*.'

But these results do not necessarily mean that the factor of attention is the *same* as the '*g*' factor. It is certain that the intensity and duration of the attention required for success (other things being equal) vary from one task to another. We may, therefore, regard the peculiar combination of intensities and durations of attention which are called for by a particular task as one of the components of the specific factors by which success in the task is gained. In other words, we may have to seek for the solution of the problems of distributed attention not in the measurement of an individual's average endowment of '*g*,' but rather in the determination of his abilities to adjust himself satisfactorily to the special circumstances of the task. But there are also other special factors operative which must be taken into account. For instance, as already mentioned, the speed and accuracy of perception is frequently influenced by the factor of 'preparedness'[1]. However, it is the task of the experimental enquirer to allow for or to eliminate the influence of factors such as these.

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[1] Cf. experiments of C. Fox (*Educational Psychology*, Kegan Paul, 1926). It is also related of a famous professor of botany that seeking a certain species of plant he went directly to a spot where, from consideration of its nature, he expected to find it growing. He found it there, but on his return, with its appearance now more clearly in his mind, he

From the point of view of the vocational psychologist, the important thing in all practical enquiries is to determine as clearly as possible what are the essential characteristics of any given activity. Among these are the conditions under which attention is to be given. Let us examine these conditions, choosing as an illustration such a task as that of cancelling O's in a printed page. The examinee is instructed what to do, and, assuming that he has sufficient general ability to grasp these instructions, he begins to search in a systematic way for the O's. There then ensues a succession of conscious stages, a succession of attentions, in which letters are recognized as 'O' (and the action of crossing it out is then initiated) or as 'not O' (when the action is inhibited). It is not necessary that all these successive attentions should be of equal intensity; the O's certainly receive attention at higher intensity than the 'not O's,' for the perusal of each line is arrested at every O that is identified. The lower the intensity of the attention to the non-significant letters, the more quickly can the lines be examined, and the number of O's crossed out can be correspondingly increased. But to maintain a high intensity of attention for one particular stimulus and to deal with all other stimuli at or near the 'margin' of consciousness is to 'concentrate' attention, in the generally accepted meaning of the term. Such concentration enters into the recall of synonyms, the completion of sentences, the classification of ideas and the like. All such mental processes appear to be linked by the general factor '*g*,' and to give quite high measures of it. It might seem, therefore, as if 'concentration' were an element within '*g*' if it be not '*g*' itself. How then does 'distribution' enter?

It has already been remarked that although Test I requires a high intensity of attention for each of the three components of the diagram, there is a *distribution* of attention in that the cognition of each component takes place separately and in succession. The normal method of distributing attention being by alternation or succession (cf. McQueen's work), it is clear that every activity which requires more than one single cognition introduces a distribution of attention. In the process of cancelling 'O's' described above, the 'natural diffusion of attention' of which Spearman speaks consists in keeping in mind the nature of the task so that the proper succession of attentions to 'O's' and 'not O's' is maintained.

saw it in many other places, until, approaching his own home, he found it growing near, and visible from, his own front door.

Looked at in this way, there are no differences in the *nature* of attention as between its concentration and its diffusion[1]; but there are differences in the *rate* of the succession and in the *intensity* of attention necessary for the purposes of the moment. Illustration is provided by the procedure of Test II. In this test the situation requires clearness of cognition or intensity of attention at crucial moments, just as in the cancellation of letters and as in Test I; but although the time intervals are fairly long, several events may occur in rapid succession, and attention must be adapted accordingly. Consequently a state of 'balance' tends to be set up in which attention to each important stimulus is given at such intensity that the sequence can be successfully maintained. It would appear that a tendency to concentration upon any one stimulus prevents such an equilibrium; hence a person who habitually thus concentrates may fail. Nor does it seem difficult to produce a similar condition of affairs in the cancellation test. If the 'O's' were to be crossed out, not in the simple sequence of their occurrence, but according to some given rule (*e.g.* cross one miss two, cross two miss one, and so on), the situation would be not unlike that above described. The person performing the task must then not allow the intensity of his attention to any portion of the whole task to interfere with the smooth performance of the remainder. Even though he carries through the complete task by a succession of attentions to parts of it (as the majority of persons will do), it is probable that a too high intensity of attention to any single part will interfere with the most successful attention to the remainder.

Hence the differences we have observed between one task and another ought perhaps to be regarded as due to differences in the *speed* as well as in the *intensity* at which successive attentions have to be given, and although the ability to give attention at the requisite *intensity* may be related to the '*g*' factor, the ability to maintain the *speed* may not. As to the succession itself, there can be no question: the so-called simultaneous performance of two or more tasks only occurs when automatisms functioning in different parts of the body are employed. (Thus a series of movements of hand and arm may accompany a series of movements of the muscles of the larynx and other vocal organs; these movements are automatic so that attention, in the sense in which we have considered it, does not enter.)

If this view be accepted, then the differences between individuals may be ascribed to the fact that they are not equally able to give attention successively at the required intensity and at the required speed. Since both these vary from task to task, there may be many different combinations of speed and intensity; there are also many different kinds of material whose intrinsic appeal differs from person to person; there are also various effects of training, practice and special knowledge; so that it is not surprising that the performance of an individual varies from task to task.

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[1] This is what Spearman means when he says that concentration and diffusion are alternative dimensions of the same cognitive output. Spearman, *Abilities of Man*, p. 269.

But it must be admitted that the tasks which have been used for such comparative purposes have differed very greatly from each other. Even Tests I and II differ considerably, although our analysis has shown that in both of them there is a succession of attentions at varying intensities. But in Test I *the rate of succession is within the individual's control*; in Test II *it is determined from without*; and unless the individual can adapt himself to the rate of succession of the stimuli he will fail. Clearly such differences are important, and, as we have seen, the correlation between these two tests is small. Yet, small though it is, it is significant and is closely related to the correlation between these tests and those of general intelligence and immediate visual memory. Even though, on Spearman's theory, these connections are to be ascribed to '*g*,' we might look upon them as representing the extent to which the ability to give attention at the requisite intensity enters into success in all of them. Consequently the fact that the correlations are not higher must be attributed to the large differences in the other conditions of the tests, particularly as regards the rate at which successive attentions are required.

In the Intelligence Group test the rate of succession depends upon the individual himself, and similar conditions prevail in cancellation, Stanford Binet (in the separate tests), Form Relations Group test, and the like. Probably the large differences in the nature of the materials employed in these tests partly account for the comparatively low correlations of these tests with each other. But as regards Test II and similar tasks in which the rate of successive attentions is outside the individual's control, this itself constitutes an important specific factor. In tapping and adding, in card sorting, and counting by threes, and in the other simultaneous disparate activities investigated by McQueen, the rate of succession of attention is very high. To some extent it is within the individual's control. But the setting up of a satisfactory 'balance' or 'equilibrium' (above mentioned) is hindered by the need for making the muscular adjustments work automatically. Consequently no large connections between the various activities investigated were discovered. It may be, therefore, that an extremely high speed of successions of attention requires a specific set of conditions which vary according to the nature of the tasks to be performed. This would explain the experimental results so far obtained.

V. SUMMARY.

To sum up, the experimental investigation of tasks which are designed to throw light upon differences in the abilities of individuals 'to pay attention' points to:

1. Resemblances (not large, however) between tasks which require for successful performance a high intensity of attention and in which the succession of attentions (if required) is within the individual's own control. Such resemblances seem to correspond to the influence of a '*g*' factor; they are also most noticeable in tasks which require a relatively high intensity of attention (or concentration).

2. Marked differences between tasks which require for successful performances (i) a succession of attentions at a very rapid rate, or (ii) a succession of attentions at a rate which is outside the control of the individual. Such tasks may be connected by the fact that a certain intensity of attention is also needed, but the resemblances are slight. Most of the 'distributed' attentions are of this kind.

3. The conclusion that specific factors are large in distributed attentions, because:

(*a*) the task calls for some special adaptation based upon knowledge, acquired skill, or special sensory and muscular equipment;

(*b*) individuals differ in sensory acuity and muscular adaptability;

(*c*) the 'natural rate' of the individual is different from that required for the task, some readjustment being necessary;

(*d*) the rate of change of attention differs from task to task.

But it is necessary to note that the majority of the tasks which have been used in experimental work on these problems belong to the class in which successive attentions, if required, are given at the individual's own rate. It seems advisable to examine other instances in which the more specialized conditions of 'distribution' obtain. There are many of these in everyday life; the practical psychologist must, therefore, devote more time to their study. It may be that tasks of the second type are to be found so closely resembling one another in the processes of attention which they require that correlations may be obtained much larger than any contained in this report.

(*Manuscript received* 14 *June*, 1930.)

TESTS OF APHASIA.

By CHARLES FOX.

I. *Do certain tests of aphasia involve the use of language* (pp. 242–243).
II. *A method of testing the tests described* (pp. 243–245).
III. *Results obtained by this method* (pp. 246–250).
IV. *Loss of language in aphasia is an indication of a more general loss of function* (pp. 250–253).
V. *Does aphasia imply a diminution of intelligence* (pp. 253–255).

I. Do certain tests of aphasia involve the use of language.

In his classical work on aphasia Dr Henry Head made use of a series of hand, eye and ear tests. The patient sat facing the physician who touched his own right or left eye or ear with his right or left hand; and the patient was instructed to reproduce the movements. Or alternatively, the outlines of such movements were drawn on cards which were placed in front of the patient with the instruction to imitate them. As the patients had to reverse the seen movement the test was difficult for them and mistakes were frequent. Amongst the variety of tests for aphasia which Dr Head employed, he makes the remark that this is the only one in which a perfectly normal person may make blunders. Anybody who has watched a squad trying to imitate the movements of their instructor will be able to confirm the fact that normal people by no means find this sort of test easy.

Now what precisely does such a test involve and is it a test for aphasia, *i.e.* does it imply the use of language in any form? Dr Head is convinced that it does. In his book on *Aphasia* (Pt II, ch. 2, p. 170) he says that when the card is in front of a person "the words 'right' and 'left,' 'eye' and 'ear' must be silently interposed between the reception and the execution of the order." Again, on p. 198, the following statement is made: "When a patient, sitting opposite me, attempts to imitate movements of my right or left hand brought into contact with an eye or ear, some degree of internal verbalisation occurs as a phase of the normal act. No sound need be uttered; but the words 'right' and 'left,' 'eye' and 'ear' or some more inclusive expression such as 'opposite' or 'the same' are essential to correct imitation of this kind....For the same reason patients may experience considerable difficulty in carrying out these tests, when the order is placed before them in the form of a picture."

If this view is correct the test is an excellent instrument of investigation for detecting language difficulties in aphasics. In the employment of any psychological test there is always a danger to be avoided known as the 'psychologist's fallacy[1].' This arises from the fact that the experimenter is apt to suppose that the subject will respond to a stimulus or an order in the same way as he himself would respond in like circumstances. The only way of avoiding this difficulty is to secure careful introspection from the subject as soon as the task is completed. Wherever possible, Dr Head took full advantage of this with his war patients. However, aphasics are not the best people to perform the difficult task of introspection satisfactorily, and it is safer to get introspective data from normal people.

The question arises as to whether the tests really do involve the use of language or whether inability to perform them correctly rests on some other defect. It occurred to the writer that the best way of solving the problem was to try the tests on perfectly normal persons and to secure introspective evidence by this means. In this way it was hoped not only to evaluate the tests from the point of view of language, but to discover what defect was present in patients who failed to perform the tests accurately.

II. A METHOD OF TESTING THE TESTS DESCRIBED.

In order to carry out a psychological investigation of this nature it is necessary to adopt a fairly rigid technique and to adhere to it with every individual subject. It is also essential to take great care to avoid suggesting to the subject what one is looking for. The subjects were University honours graduates and certain schoolboys. In dealing with the graduates the experimenter said: "I am going to show you some pictures which suggest movements and I want you to perform these movements." To the schoolboys the instruction was: "I am going to show you some pictures and I want you to do just what the man is doing in the picture. Do you understand?" Head's diagrams drawn to the size of a postcard were then placed, one by one, on a table before the subject, who sat facing the experimenter. The card remained on the table until the subject had performed the movement, when it was withdrawn and immediately afterwards the next one was exhibited; and so on through the series. The order of presentation was the same in every case, and nothing was said until the whole series was completed. The series is shown here for reference (reduced in size) in the order employed. When the whole eight had been shown they were repeated in the same order; making sixteen tests in all. As soon as the series was completed the following questions were asked and the replies recorded verbatim: (1) "Was it easy?" (2) "Tell me how you did it?" In case the answer to question (2) proved unsatisfactory in getting a full introspective account, the question (3) was asked: "What did you think of before you did any one?"

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[1] See C. Fox, *Practical Psychology*, Introduction, p. xv. Kegan Paul, 1928.

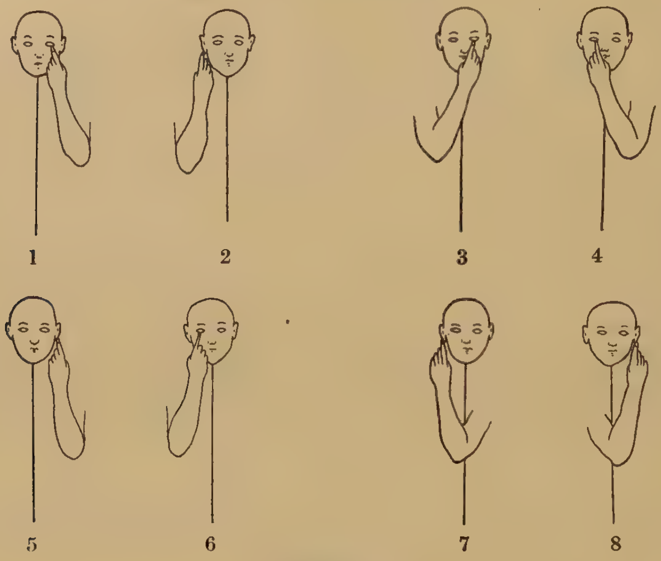

Diagram I.

Certain of the schoolboys did the whole series incorrectly, reversing every movement, *i.e.* they ignored the fact that the picture as they see it must be turned round in imagination before they can perform the movements. Such cases are purely imitative and will be referred to as 'automatic'; and for these the following questions were asked after the task was completed. (1) "Did you do them right?" If the subject asserts that he did them correctly, he is shown Diagram I, 1 again and told to repeat the movement. Then, if he is still incorrect, he is asked (2), "Are you doing just what the man is doing?" If he persists in saying that he is, the final question (3) is asked, "Which arm is he moving?" By this means it was possible to discover in practically all cases what method was employed. More especially it was possible to test Head's view that language is an essential part of the process.

Now if we consider the problem that the subject has to solve we realize that there are three possible ways of doing it. He may give himself a verbal order by the use of 'internal speech,' such as "I must do the opposite," or "I have to use the opposite hand," or "The left hand on the picture corresponds to my right"; and this was the method used by some of the graduate subjects. But the problem may be tackled by spatial manipulation only, without the use of internal speech; and this may take various forms. The person may imagine that the diagram is turned round, and consider himself as looking through the card from the back as though it were transparent. Or he may imagine the card placed behind him so as to face the same way as he himself is looking. It would usually be said that this involves the use of visual imagery, but if any imagery is employed it is so vague as hardly to be recognizable. The ability to think of objects as turned round is acquired so early in life that, if images were originally used, they have long since disappeared from consciousness. For images tend to disappear as soon as they have accomplished their purpose of making thinking facile; and when thought flows freely images play no part in the process[1]. We may describe this procedure of spatially manipulating the card in imagination as 'orientation of the diagram.' Finally, it is possible to solve the problem by means of orientation of the body, and this may be accomplished in a variety of ways which it is difficult to separate. The more usual method involves the use of kinaesthetic imagery or incipient bodily movements. The person imagines his own body taking up the position represented on the picture. It is very doubtful whether pure kinaesthetic imagery has any independent existence, or whether in all such cases incipient bodily movements are alone involved. The spatial schema of our body which we all carry about with us in consciousness is more or less easily manipulated. Some subjects aver that they 'feel' their bodies turned round in space so as to be in the position of the picture. Others 'feel' nothing, but simply 'know' that their bodies have made the necessary turning movement. Careful observation of the subjects discloses the fact that incipient movements of turning the body are made; and in some cases the movement is very pronounced, so as to constitute a quarter or half a turn of the body in the chair. This is more especially the case with children, and I have observed some schoolboys who actually got up and turned completely round in their effort to solve the problem. The orientation sometimes takes a visual form, and the subject 'sees' himself turned so as to occupy the position of the man in the sketch. For purposes of discussion it is convenient to refer to all the cases of this last group, namely kinaesthetic, incipient or actual movements, or visual realization of movement, as 'orientation of the body.' The above analysis is based on the actual procedure adopted by the persons who were examined.

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[1] C. Fox, "Conditions which arouse images in thought," *Brit. J. of Psychol.* VI, General Section.

III. Results obtained by this method.

We shall next consider the different groups of subjects on an age basis. There were twenty-one honours graduates, all of whom were members of a practical class in psychology and had, therefore, some experience of introspective methods. It was clear from the introspections, obtained by dictation from each person immediately after he was tested, that some of the subjects used a mixed method; and probably closer investigation would show that this is more common. As the same person may use a variety of methods, the numbers in the different categories named below will not correspond to the number of persons tested. Twelve subjects oriented the body, eleven used a verbal method (internal speech) and five oriented the diagram. Only four of them succeeded in doing the whole series without an error. The errors were distributed as shown:

Errors ...	0,	1,	2,	3,	4,	5
No. of subjects	4,	5,	7,	1,	2,	2 = 21.

In some cases the mistakes were corrected immediately they were made, so that out of a total of 40 errors 10 were corrected spontaneously. In some other cases the mistakes were realized but not corrected.

The next group of subjects consisted of forty boys from a selective Central School, *i.e.* a school which selects its pupils by competitive examination from elementary schools. Their ages ranged from about 12 to 15 years. In this school the forms are classified as bright, medium and dull by the headmaster. There are four bright classes and four dull; and the five cleverest boys from each of the bright classes, and the five stupidest from the dull classes acted as subjects. Introspection records were made for each boy in the manner indicated above, and the analysis here given is that for the brightest boys. There were eleven out of the twenty who oriented the body, ten employed words (internal speech) and three oriented the diagram. This is very similar to the distribution found amongst the graduates. But with these subjects a new phenomenon presents itself; for there were three who were completely automatic. Only one succeeded in doing the whole series without an error. The errors were distributed thus:

Errors ...	0,	1,	2,	3,	6,	7,	10,	16
No. of subjects	1,	2,	5,	6,	1,	1,	1,	3 = 20.

Excluding the automatic cases, there were 53 errors of which 13 were spontaneously corrected.

The results obtained from the twenty dullest boys in the school show a different state of affairs. The striking feature of this group is the large amount of mimicry, for nine of the twenty were purely automatic and another was practically so. There were five who oriented the body, six were verbal (internal speech) and three oriented the diagram. The errors were distributed thus:

Errors ...	0,	1,	2,	3,	4,	5,	7,	13,	14,	16
No. of subjects	0,	2,	1,	2,	2,	1,	1,	1,	1,	9 = 20.

Of the 57 errors made by the eleven non-automatic subjects 13 were corrected spontaneously. There were indications that the automatic group were easily suggestible.

In order to see what would be the result of defects of speech on the ability to do these tests all the boys who stammered were tested. Only nine cases were found in the school, eight of whom stammered only when reading or excited, and one stammered badly. Four out of the nine were completely automatic. Of the rest three oriented the body, two the diagram and there were none who used internal speech. The number of cases is too small to draw any certain conclusion, but, as far as they go, they tend to show that a difficulty in the use of speech makes this group of stammerers approximate in its performance to the group of dull boys. Speech deficiency has the same effect as dullness.

It is interesting and valuable to discover what is the lowest age at which the problem presented by these tests is capable of solution. For this purpose thirty-three boys from a good elementary school were examined. They were carefully selected by the headmaster, twenty-three being the brightest for their age in the school, and ten the dullest.

The dull boys had an age range from 11 years to 9 years. They all understood the problem and carried it out without hesitation. Many of them were very prompt, doing it immediately. Nevertheless not a single boy did any test correctly. It appears, therefore, that for dull pupils the problem is too difficult for accomplishment at 11 years, but we saw above that it can be accomplished by them at the age of 12. Six of the subjects slavishly copied the diagram and consequently reversed every movement; they were purely automatic. In the other four a similar state of affairs was evident and they, in addition, failed to notice in three or four of the tests that the hand should have been crossed over the body.

With these boys Diagram I, 1 was shown again at the end of the series, and they were asked to repeat the movement. Each did it incorrectly again. Then, whilst his hand was in the incorrect position, he was asked whether he was sure that what he was doing was correct. Three of the boys realized that the movement they had made was wrong. Six of the others insisted that the position was correct, and only realized their error when they were asked to state in words which hand the man in the diagram was using and which they were using. The remaining subject declared that the diagram displayed the right hand. I repeated the movement myself, and he still maintained that the right hand was used. When I turned my back on him he said it was the left hand, but on turning again to face him he declared it was the right again. It is obvious that this boy only thinks of right and left with reference to himself, and has not yet learnt to think of these positions with regard to some other object. In this he clearly shows what defect some aphasics suffer from, namely, an inability to conceive spatial relations correctly.

The twenty-three bright boys may be separated into two groups, (*a*) and (*b*), according to age. The first group consisted of ten boys whose ages ranged from 11 years to 9 years and the remaining thirteen had an age range from 8 years 5 months to 7 years 9 months.

In the former group there were three who oriented the body, four who imagined the diagram turned round, one of whom also used internal speech. The remaining three were automatic, but two of them realized their error when they were shown Diagram I, 1 again, and were asked to put the problem into words. None of them corrected his errors spontaneously. The errors were distributed thus:

Errors ...	0,	1,	2,	3,	4,	8,	16
No. of subjects	0,	1,	1,	2,	2,	1,	3 = 10.

The only thing which differentiates this group of younger bright boys from the previous group of older bright boys is the inability to correct the errors spontaneously. In other respects they are similar.

We may say, then, that the ability of clever boys to perform this series of operations does not vary appreciably between the ages of 9 to 15 years. And, if we exclude the cases of pure imitation, there is little difference between the youngest boys and the University honours graduates. The spatial ideas involved in performing this sort of problem and the formulation of it in internal speech, where this is employed, are abilities which are fully developed in clever boys at the age of 9.

When we turn to the younger group of bright boys we meet with a different situation. All of them turned out to be purely automatic, for in every one of the thirteen cases there were 16 errors, and practically all the movements were reversed from right to left or *vice versa*. Seven of the subjects failed to cross the central line of the body in two or three instances where the tests demanded it. Each boy, as before, was shown Diagram I, 1 again at the end of the series, and when he had again performed the wrong action he was asked to state in words what the man was doing. Seven of them realized their error, and corrected it when they were thus forced to formulate the problem in words. In other instances this could not be done, and the errors persisted.

Three cases are instructive and may be quoted more fully. A boy of 8 years used his right hand instead of the left, and insisted that the man was doing so. On being pressed he changed his ground and said the man was using his left hand. Then he said: "I forget which is right and which is left." When he was asked to show his right hand he showed his left and *vice versa*. Another boy of 8 years maintained firmly that the diagram's left hand was the right. When I repeated the movement sitting opposite to him he also said I was using my right hand. He could only be persuaded to the contrary by standing behind my back whilst I was performing the movement. A boy of 7 years 9 months who had made 16 errors, which included six cases where he had failed to cross the central line of the body, was asked to show his left hand and his right eye, which he did perfectly well. He was then shown Diagram I, 1 again and did it wrong, as before. He used his right hand, and insisted that the diagram was doing the same. Nevertheless, although he could point quite easily to my right or left hand, he invariably confused these when shown a diagram which was in the same position with regard to him as I was. This case clearly shows that there is a greater difficulty in symbolic formulation when interpreting a simple diagram than when interpreting the movements of a person placed opposite to oneself.

We may conclude that the spatial ideas underlying the sort of problem involved in Head's tests, and the ability to formulate symbolically the meaning of a simple abstract diagram, do not become habitual with clever boys until the age of 9 years and with dull boys until the age of 12 years[1].

For comparative purposes I have analysed the clinical records of Head's war patients given in *Aphasia*, vol. II. There were sixteen of such cases examined by the present set of tests, but unfortunately only thirteen had the whole series of sixteen cards. The errors were distributed thus:

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[1] I was unaware until this paper was finished that Head had anticipated this result. Thus he says, *Aphasia*, I, 396, "Asked to imitate the movements of the examiner, when face to face, he may give up all attempt to employ the right or left hand correctly and consistently select the one that is opposite. Power to carry out this test accurately is acquired between the ages of nine and twelve, and the aphasic reverts to the simpler method of response habitual in childhood." Head gives no indication as to where he got his figures.

Errors ...	0,	1,	2,	3,	4,	5,	6,	11,	12,	13,	15,	16
No. of subjects	0,	1,	1,	1,	1,	2,	1,	2,	1,	1,	1,	1 = 13.

We may safely assume that the patient with 15 errors was completely automatic, so that this group contains two automatic cases. Excluding these, there were 73 errors of which the records show that 5 were spontaneously corrected. The distinguishing feature of the group compared with the graduates and the older schoolboys is the greater inability to correct errors spontaneously. It must be remembered that Head's procedure was far less rigid than mine, and there is nothing to indicate that the instructions given to his patients were invariable. If a rigid method had been adopted, it is probable that the total number of errors would have been greatly increased. Moreover, each of his patients had preliminary practice in the simpler task of imitating the movements of the physician before he was tested with the cards; which would tend greatly to reduce the number of mistakes. Despite this, the mistakes are much greater relatively than in the other groups, and there is a greater proportion of cases with a considerable number of errors. The surprising feature of the group, however, is that the number of automatic cases is very much smaller than we should have expected, and this is probably due to the preliminary practice just mentioned. Whatever particular intellectual defect these patients sustained as the result of their wounds did not reduce their mental level for this task below the age of 9 to 12 years.

IV. LOSS OF LANGUAGE IN APHASIA IS AN INDICATION OF A MORE GENERAL LOSS OF FUNCTION.

We may now attempt to get an answer to the question we started with, namely, whether inability to cope with the tests is due to some defect of speech or to some other deficiency. It has become a commonplace in modern psychology to argue from the abnormal to the normal. Such a method is beset with innumerable pitfalls, and it is debatable whether the procedure is ever really valid. We are on much firmer ground if we travel in the contrary direction and reason from the normal to the pathological. It would be a great benefit if all clinical tests were first of all tried out on normal people, so as to estimate the deviations from normality in pathological cases. This was the method adopted by Alfred

Binet in securing tests for mental deficiency. He standardized his tests by means of normal children, and then tried them on backward and deficient children. That his method was the sound one is shown by the immediate fruitfulness of his work and the astonishing growth in the use of mental tests since his time; all standardized in the same way.

If we turn to our graduates and the older brighter schoolboys, we find that, out of a total of forty-one subjects in these two groups, twenty-one employed internal speech in order to carry out the tests. But of the twenty-one, fourteen also made use of some form of orientation, and the introspections showed that the speech was merely subsidiary to the other method and was not constantly employed. In many cases speech dropped out after the first couple of tests. With the remaining groups speech was much less frequently used. To a limited extent, therefore, Head's view is justified, namely, that speech *may* be a part of the process used in doing the tests. But his contention that speech is essential to the process, which cannot be performed without it, is not borne out by our investigation. Speech is merely an incidental aid used only rarely.

If anybody is accustomed to walk up a street one way, he will usually find considerable difficulty in interpreting a photograph of the street taken from the opposite way; and the same difficulty may be observed in any picture taken from an unaccustomed angle. He who has experienced this difficulty will easily realize the essential nature of the problem presented by these tests. It is a difficulty of orientation or spatial discrimination. And it is worthy of note that some of the other tests of aphasia employed by Head, namely the clock tests and the coin-bowl tests, are fundamentally tests of spatial relationships. Some of Head's war patients suffered from grave affections of orientation as shown by the observation that they could not set out the plan of a familiar room or find their way about in familiar surroundings.

Referring again to the group of forty-one persons just mentioned we find that, whether internal speech was used or not, there were thirty-one cases of orientation in carrying out the tests; either orientation of the body or the diagram or both. So that the hand and eye tests are also to a considerable extent tests of the power of discrimination and manipulation of spatial relations.

I am inclined, therefore, to agree with Dr R. Mourgue[1], who thinks that aphasia is a special case of a more general disturbance of function. According to him it is due to a general weakening of the "fonction de découpage et d'opposition de l'intelligence." By this he means an inability to demarcate or differentiate, which shows itself in a lack of appreciation of spatial relationships. He thinks that the varieties of aphasia are reducible to special disabilities in spatial discrimination, and that language is only disturbed because it too involves such demarcation. It is evident that the hand, eye, and ear test, at the very least, involve some sort of spatial perception. Accordingly Dr Mourgue maintains that, in the majority of cases, no language is necessary, but that those who make mistakes experience a difficulty in perceiving spatial relations. Errors are due to the fact that "le sujet ne peut exécuter la transposition spatiale nécessitée pour l'imitation des mouvements de l'expérimentateur placé devant lui." Our experiments show that for normal subjects this view is well substantiated.

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[1] R. Mourgue, "Disorders of symbolic thinking," *Brit. J. of Psychol.* Medical Section, 1921, I.

Further confirmation has been provided by the work of W. van Woerkom[1] who made a careful study of the restitution of function in one of his aphasic patients. For the first 2 weeks this patient was dumb, but after 1 month he was able to name objects and understand speech and thenceforward improved. His process of re-education was so complete, that, after 6 weeks, a superficial observer could have detected nothing abnormal about him except that he expressed himself with difficulty. Nevertheless a careful examination showed that there was profound loss of function. He could not place a ruler parallel to another at a short distance, but crossed them fumblingly; nor could he imitate the directions of a stick moved in the air. With his eyes bandaged he failed to imitate passive movements impressed on his limbs. He could not place an object correctly on one side or another of a stick even if he was shown what to do. He could point successively to two objects in front of him, but could not indicate their spatial relation either by words or gestures. In fact his geometrical sense was abolished almost completely.

He could carry out purely habitual acts, such as lighting a cigar, since these do not involve any idea of the movement to be made, but are secondarily reflex activities. What he lacked was the idea (the representation) of movement. As a result of this inability "il échoue complètement dans les épreuves plus compliquées, lorsque, placé en face de l'observateur, il doit imiter ses gestes, se mettre mentalement à sa place."

Since a good deal of our ordinary thinking involves the manipulation of spatial ideas, which, as the result of our education, have become unconscious, it follows that Head's tests are, to a certain extent, an indication of the mental level of the patient. Our experiments on school children bring this out clearly; for the difficulty of the task is such that bright children of the age of 8 years, who understand what is required of them, are nevertheless unable to cope with the problem. The ability to manipulate spatial ideas of this kind is an indication of the mental level attained by bright children at the age of 9 years. Duller children even at the physical age of 11 have not yet succeeded in mastering the task. For this particular problem a dull child of the age of 11 is at the same intellectual level as a bright child at the age of 8. Head's tests, therefore, form an exceptionally good means of gauging the intellectual disturbance in spatial ideas of patients suffering from aphasia. They provide an admirable method of measuring the exact stage of restitution of mental functioning in so far as this is concerned with the recovery of the spatial notions which lie at the base of so much of our thinking. They are probably more suitable to the advanced stages of re-education than to the exploration of the loss of function at the beginning of aphasia; and indeed Head partly employed them for this purpose, as he was in the habit of testing the same patients with the tests over a series of years.

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[1] W. van Woerkom, "La signification de certains éléments de l'intelligence dans la genèse des troubles aphasiques," *J. de Psychologie*, 1921.

V. DOES APHASIA IMPLY A DIMINUTION OF INTELLIGENCE.

The concept of aphasia has undergone many revolutionary changes and the present need, from the scientific standpoint, is for a more accurate differential diagnosis of its varieties. It seems to me that this could be secured if all the other serial tests employed by Head were standardized by being tried on school children; such as the naming of common objects, the naming and recognition of colours, the clock tests, the coin-bowl tests, etc. Many of these obviously necessitate the use of language, whilst others, as clearly, involve chiefly the manipulation of spatial and other relations. The purpose of the investigation should be to discover the lowest age at which the majority of normal children can cope with each serial test successfully. As it happens, a test for the naming of colours has already been investigated, for Binet employed it in drawing up his metrical scale of intelligence and it is now regarded as a test for the mental age of 5. Again, the ability to distinguish the right and left sides of the body, which is much simpler than the tests we have investigated, has been assigned to the mental age of 6[1]. In this way, by adopting the method used in the present research, a mental age could be assigned to every test and a scale drawn up; and it would then be possible to discover the precise degree of defect that different aphasics suffer from, and to estimate exactly the degree of restitution of function during recovery. The employment of graded tests by Head has considerably advanced our knowledge and the next logical step is to refine the graded tests by standardizing them.

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[1] Terman, *The Measurement of Intelligence*, ch. 12. Harrap.

The plan here suggested of determining the mental age of aphasics with regard to the various serial tests raises the difficult question as to whether the general intelligence of such patients is impaired. This obviously depends on what we mean by general intelligence. There is no doubt that several aphasics are highly intelligent people in spite of their handicap. One of the most striking examples is that of Dr Saloz, who was suddenly attacked with aphasia at the age of 60, and completely lost his powers of speech and writing[1]. On recovery, which was gradual and brought about largely by a deliberate and determined auto-education, he wrote an account of his own case. Immediately after his seizure he was able to make a correct diagnosis by pointing to his left temple to indicate to others what was the nature of the complaint. He says that he knew exactly what he wished to say, but had lost the symbols of expression. "Je me sentis comme muré dans un sépulcre." He had all his conceptions, but very limited means of expressing them to others. His thoughts, sentiments and volitions were intact, but there was a hiatus between his world of ideas and the world of words. His account reveals a very high degree of critical acumen, but, strangely enough, he sums up the various disabilities from which he suffered, thus: "La somme de tous ces déficits...équivaut à la perte ou à la faillite de l'intelligence." M. Naville, his friend and physician, says that, when he recovered, his intellect was not as supple as before, for he frequently adhered to a first opinion and had difficulty in following two ideas at the same time: defects which most of us share with him! M. Naville concluded that aphasia is not the result of diminished intelligence, but is characterized by the inability to employ a special mechanism that intelligence has created as its chief working tool.

Nevertheless, this view is not completely satisfactory for, after all, intelligence must employ some means of expression and the inability to make use of the recognized tools is *ipso facto* a loss of aptitude. We may best express this in the phrase employed by Hughlings Jackson, that the loss of the power to speak and write make a man "lame in thinking." There are, of course, alternative methods of arriving at a result; a child who cannot say what twice four are may, nevertheless, reach the answer by counting up on his fingers. Yet the inability to use the multiplication table would normally be regarded as a sign of a lower mental level. And our investigation has shown that what distinguishes the bright boys, *i.e.* those with most general intelligence, from the dull is the greater ability to deal with spatial relations. As there are a variety of other relations manipulated in thought the ability investigated is relatively specific. If there were no specific forms of intelligence there could not be such an entity as general intelligence. I am, therefore, in entire agreement with Dr Head who has summed up the situation lucidly[1]: "All the processes which suffer in aphasia are essentially psychical and, in so far as they are defective, the capacity of the mind is restricted. Yet it is not general intelligence which is primarily affected, but the mechanism by which certain aspects of mental activity are brought into play. In proportion as these forms of behaviour are necessary for the perfect exercise of psychical aptitudes 'general intelligence' undoubtedly suffers....Should the patient be unable to carry out a task in a normal manner, he falls back on a simpler and more childish method of solving the problem....But as with many children who are supposed to be mentally deficient, the loss of intellectual capacity is specific and not general."

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[1] Naville, "Mémoires d'un médecin aphasique," *Archives de Psychologie*, XVII.

A complete standardization of the serial tests of aphasia would help to determine the degree of loss and recovery of these specific capacities, and make the diagnosis more exact by providing a psychograph of each individual case.

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[1] H. Head, *Aphasia and Kindred Disorders of Speech*, I, 418. Cambridge University Press, 1926.

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THE INTERPRETATION OF CHILDREN'S DRAWINGS.

By C. A. OAKLEY.

(*From the Department of Psychology, Glasgow University.*)

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I. INTRODUCTION.

Two hundred investigations have been carried out in the field of children's drawings. They have produced little of practical value. Few have been analytical in method, most remaining satisfied with the airy pleasantness of descriptive writing. As a result spontaneous drawing continues to play little part in the standard systems of mental testing—an unfortunate position in view of its closeness to the child's interests, its relative freedom from the extraneous influences of both training and the personality of the examiner, its non-verbal character, its simple and ready application.

Summaries of these published works have been given by Ayer (*Psychology of Drawing*, 1916), Goodenough (*Psychological Bulletin*, 1928) and others. Recently there has been a renewal of the efforts to devise objective scales for the measurement of children's drawings, and it is with one of these attempts that I am at present associated. In the absence of standardized material, the collection of which must be a matter of some years, it has been suggested that the subject might lend itself to a discussion on the interpretation of these drawings.

II. EXISTING SCALES

Scale-making has been approached from two directions—comparison and analysis (although it has been maintained that the former consists merely of doing crudely that which the latter seeks to do accurately). In methods of comparison the standards are median samples, there being generally one sample for each year or part of a year. Sometimes a series of samples is employed to each of which has been assigned a numerical value. In this case the drawing of the child under examination is marked, and by the means of an age-scale of normal scores, a (drawing) mental age is estimated.

On the other hand, for analysis, the product is resolved into its constitutive elements, and a score is based upon the quantity and quality of the details appearing in the child's drawing. A point-scale is arranged with a normal score for each year or part of a year.

Existing scales based upon comparison include those devised by E. L. Thorndike ("The Measurement of Achievement in Drawing," *Columbia Teachers' College Record*, 1913), by Cyril Burt (*Mental and Scholastic Tests*, 1921), by L. W. Kline and Gertrude M. Carey (*A Measuring Scale for Freehand Drawing*, John Hopkins Studies in Education, 1922), and by Stella McCarty (*Children's Drawings*, 1924). The only published example of the application of analytical method is that given by Florence Goodenough (*Measurement of Intelligence by Drawing*, 1926).

III. WHAT CHILDREN LIKE TO DRAW.

Each and every investigator has faced the fundamental difficulty of ascertaining the most suitable subject to be drawn by the child. Dr Beatrice Edgell has maintained that, when measuring the child's intelligence, we are on sound ground only when the tests follow the lines of his distinctive interests. This point is particularly applicable in the sphere of drawing.

Many investigators have followed Mrs Maitland (1895) in endeavouring to tell us what children like to draw. They are agreed that, in the earlier years, the subject of a human being predominates. (Ivanoff has called attention to the significant fact that of these representations 95 per cent. are those of adults and only 5 per cent. those of children.)

Typical of these general classifications is that given by Lukens (*Pedagogical Seminary*, 1896) for children of age up to 10 years:

	%
Human beings (or, including those in composite pictures, 75 %)	44
Illustrations of stories	12
Animals	7
Plants	3
Designs, etc.	2

Ballard has considered this question very thoroughly for children in South London and Glamorgan. He found that the boy's favourite object for drawing is a ship, a girl's being plant life, followed by houses. Ballard noticed that the age of loss of interest in drawing human beings is 9, there being a second falling off after the age of 12.

The work of these investigators may be summarized to bring out the following points:

(*a*) The child's favourite subject is that of a human being. (Drawings of ships, toyshops, etc., generally contain representations of people.)

(*b*) Houses and plants (for girls) and various mechanisms (for boys) are other popular subjects, drawn for their intrinsic interests and not as part of the illustration of a theme.

(*c*) Composite pictures take generally the form of either a representation of a favourite scene or an incident in a story.

(*d*) Environmental conditions influence the child's choice of subject. Ballard has shown that both Glamorgan and London boys draw ships. The same seems true of most British boys with their heritage of the sea traditions. But many investigators in other countries do not even include ships among the subjects drawn by boys.

The significance of social praise is discussed later.

IV. THE SELECTION OF SUBJECTS.

Only one investigator (Thorndike) has left the child free in his choice of subject. Constraint may be exercised in various ways:

(i) *Limitation to one subject.*

Both Burt and Goodenough restrict themselves to the examination of the child's drawing of a man and, if one subject only is to be employed, then this choice is more than justified. Other possible subjects include those of a man in character (for example, a postman), a dog, and a horse and cart. (The theme-illustration test, such as that given by Earl Barnes, is not considered. There is little record of any tests of this character being adequately standardized. Childs (*Journal of Educational Psychology*, 1915) gives an interesting test of this description, in which he employs a Thorndike Scale.)

(ii) *Utilization of more than one subject.*

The Kline-Carey test employs four objects—a house, a rabbit, a boy running, and a tree. McCarty limits herself to three—human beings, houses, and compositions involving trees. A numerical value is determined from the samples for each of the three drawings. The mean is calculated, giving the child's final score.

If we wish to employ the child's drawings to give us a measure of his intelligence, then, following Dr Edgell's line of criticism, we must dismiss the limitation method as inadequate. It favours the child who is accustomed to draw men.

In this paper, however, I am dealing only with one subject—that of the drawing of a man—and I leave further discussion of this point till a later page is reached.

V. INTERPRETATION OF THE SCORES.

Any discussion on the significance of a mental age based upon drawing must begin with an attempt to determine the extent to which intelligence is the basic factor.

Some interesting correlations between Binet (Stanford Revision) M.A.'s and Drawing M.A.'s are available. They may be tabulated in the following way:

(*a*) *By the Goodenough scale.*

Age	No. of cases	Correlation	P.E.
4	25	·863	·034
5	94	·699	·035
6	65	·832	·025
7	63	·716	·042
8	27	·557	·092
9	37	·728	·053
10	23	·849	·041

(*b*) *By the McCarty scale.*

4·0 4·6	47	·106	·097
5·0	183	·244	·047
5·6	239	·364	·038
6·0	308	·623	·024
6·6	214	·455	·037
7·0	220	·368	·039
7·6	107	·082	·065
8·0	75	·634	·047
8·6	27	·572	·088

The correlation is thus definitely positive, but the relationship, particularly in the case of the comparative method, is not of a high degree. Results obtained by the present writer are of the same order, and lead him to suppose that, speaking very generally, a correlation of between ·5 and ·6 is to be expected. A new scale is being standardized at the moment, but here again early indications are that no close correlation is obtainable. It would seem therefore that drawing tests do not provide us with a satisfactory measure of intelligence (assuming, of course, that the Binet tests themselves do so).

VI. How do children learn to draw?

Two questions must be answered: (*a*) How does a child learn to draw? and, the more fundamental, (*b*) Why does he draw? Records of various attempts to analyse the drawing operation are available. Albien (*Zeitschrift für experimentelle Pädagogik,* 1907) distinguishes two distinct major processes, optical-perceptual (leading to the unconscious fusion or contrast of related ideas) and motor graphic. He analyses these processes and details the various factors which enter into them. Except for the emphasis upon visual imagery, there seems little in these processes which is not basic to, and constitutive of, general intelligence. He distinguishes also two drawing types:

(1) The visual type with two sub-types: (*a*) The good copy-drawer, whose visual image is clear, but transitory. Little 'apperception and association' enters into the activity. (*b*) The good drawer whose work is based upon analysis and subsequent synthesis.

(2) The constructive type which relies less upon visual imagery than upon reflection and subjective construction.

These have been compared to Messmer's objective (fidelity) and subjective (interpretative) reading types.

Spearman has shown that, while the eductive processes correlate highly with 'general intelligence,' the reproductive processes do not. As in the case of so many other writers, he confesses that he is baffled by the wide discrepancy which experimental results show between tests of visual discrimination and intelligence (*The Abilities of Man*).

In his earlier volume (*The Nature of Intelligence*) Spearman discusses imagination. He maintains that the elements distinguished out of the unity of completely undifferentiated confusion are apprehended by virtue of the second principle of eduction in relation to each other. The secondary unity does not spread itself evenly over the whole mental field. It settles in unrelated patches and, of these patches, the most conspicuous in early development is that of vision. This unity, says Spearman, would appear to be the main fact responsible for the "seeing of a picture from the very first as a whole."

He dismisses the extremist's conception of imagination as fashioning spontaneously out of nothing and as being emancipated from all laws. "The imagination is asserted by the majority of professed psychologists to do really nothing more than bring old cognitive elements into new combinations. Even the newness of the combination is represented as being merely the chance effect of associative reproduction." Certainly the source of cognition must be experience.

It is necessary, therefore, that the drawing processes should be examined in the light of Spearman's qualitative and quantitative principles which delineate respectively the cognitive operations and the extensity of their execution.

In the meantime, the writer assumes that the general level of the testee's intelligence must affect considerably his success at drawing. The immediate problem facing him is the explanation of the low correlation.

VII. WHY DO CHILDREN DRAW?

Meumann (*Vorlesungen zur Einführung*, 1907) points out that few people are sensible to the inaccuracy of their knowledge of the form and colour of the most familiar objects. The syncretism of which Piaget writes is remarkably evident in the confidence which we display in the exactness of our visual images. Among the possible explanations of this inaccuracy distinguished by Meumann are:

(1) The non-arousal of the will to analyse.
(2) The presence of the will, but the absence of the ability.
(3) The lack of schemata.
(4) The lack of knowledge.

Thus attention is called to the conative aspect of the question. If the child is not interested in a subject he will show little desire to analyse his visual impressions of it, and the quality of his drawings will be poor in comparison with that of those of subjects which do mean something to him.

Accepting as valid the argument that a child will only draw well those subjects which have interested him sufficiently to lead him to study and to analyse them, we have still to explain why he should want to *draw* them.

Interest in telling something is the motive which inspires all good drawing, say Sargent and Miller (*How Children Learn to Draw*, 1916).

This position is generally accepted. A child draws to express his ideas, drawing being a primitive and universal mode of expression.

Much has been written concerning the intuitive expression to be seen in children's drawings. Indeed they are quoted frequently as evidence of the intuitive origin of aesthetic appreciation. A very full discussion on child drawing and art is given by Helga Eng (*Kinderzeichnen*, 1927).

Looking at pictures plays an important part in the first years of childhood. We have the authority of Stern for stating that, at the age of 9 months, a child can recognize pictures. At 10 months, Eng's niece began to scribble. Pictures, says Stern, incite and guide intellectual processes.

Paullson (*The Creative Element in Art*, 1923) goes further: "The impulse in drawing is the pleasure derived from the objectification of emotion." When the child sees a picture 'At the Seaside,' he re-lives affectively his recent holiday. This suggests that the child who draws a toyshop or a ship is seeking to give completeness to his imagery. He might turn, of course, to the illustrations in his picture books, but this he does only when the books thrust themselves upon him. The child is never a good searcher.

Quite early in life drawing becomes a means of gaining social approval. A child will repeat with unvarying monotony a drawing of a flower which originally earned for him some praise. In this way the desire which first prompted him to sketch the flower may be quite superseded by the impulse of an affective memory.

It would seem that only during the years coming between the ages of 6½ and 10 does the child use drawing for expression. During this period he attempts to express himself to himself. In this his work does not differ materially from art proper although, in practice, the latter is very much distorted by the influences of social approbation.

At the best, however, the normal child finds the process a rather disappointing one. The number of unfinished drawings in childhood must exceed considerably the number of those completed. Sargent and Miller discuss as a possible reason for the fading of the early promise of childhood into apathy the growing realization of the crudeness of the representations. This is very noticeable after the age of 9 years has been reached. The disappointing results achieved in graphic expression seem to lead to its total abandonment during adolescence in favour of linguistic expression.

VIII. SCHEMATA AND ASSOCIATED DETAIL.

All drawings as made by the youthful scrawler and by the polished artist are built up on experience. Their existence is an objective fact. The visual schemata upon which they are supposed to be based is still merely conjectural.

It has been pointed out that drawing may be a motor rather than a visual activity, and a definite analogy between drawing and writing would seem to be permissible.

The nature of imagery is controversial ground. The accuracy and completeness of any explanation of the drawing act must depend upon a clarification of the general position. In the present instance the existence and fundamental importance of schemata, both visual and motor, is postulated.

It is suggested also, and our own introspection, unreliable as it may be, would seem to give some justification, that the dominating factor in the drawing process is the actual figure which is developing upon the paper. In the course of time, no doubt, if the same drawing is repeated indefinitely its execution becomes as mechanical, in the motor sense, as is walking up the stairs.

It is true also that an urge to create—a possible manifestation of self-assertion—prompts the child on the first occasion to scribble, and that the imitative action with the pencil is motor, not visual. But it is felt that only by reference to visual factors can one attempt to account for such unquestioned facts as the incidence of the mixed profile, the general stages of development in drawing, and the 'copying' of details which is a source of constant vexation to the examiner who would make of drawing a group test.

Thus it is said that, while drawing is based upon both visual and motor schemata, the former is predominant. Even in the case of the child who draws one standard figure with monotonous, and presumably mechanical, regularity, there is constant reference to its visual appearance as it passes through the various steps towards its completion.

The point must be left, however, in the absence of experimental and statistical evidence. In the meantime, it may be said that while some information is available on the character of the visual schemata, little is known of the motor schemata. But, just as the child's abilities are adequate for the demands made in the learning of the writing process, so are they in drawing. As is suggested above, the child before adolescence is not critical of the quality of his own work. He fails frequently to derive the satisfaction from it which he set out to achieve. He destroys, erases, and does not complete his work. But does he blame his own incompetence for this, or look upon drawing as a game not worth the candle?

The child analyses, in the light of his existing schemata, details in drawings, pictures and illustrations, which catch his attention. The synthesis of the schemata seems to take place as the child makes his drawing. The resulting visual schema tends to become more detailed. The point may be illustrated. It may be supposed that at the present time a child's schema of a man is as in Fig. 1.

In addition there is associated detail, the product of more recent analysis, not yet stamped into the schema. Its appearances fluctuate, and the final drawing may be as in Fig. 2.

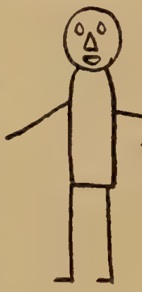

Fig. 1.

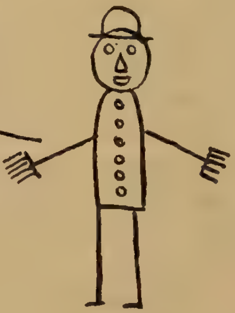

Fig. 2.

I suggest, therefore, that the child's drawing of a man is not based upon an analysis of reality. Its symbolism originates in drawings made by other people. The subject itself arises in the environment. The drawing of a man is at first simply a standard form of creative play, willingly accepted by the conservative child.

IX. GROWTH OF THE SCHEMA.

We have to explain accordingly the child's manifest liking in general for pictures, and, in particular, for those which are drawn for him. Various discussions on this point have been published, but none can be regarded as really conclusive. Different factors may be mentioned—the close social contact involved, the child's fascination with the rapid development of the figure, his self-assertive delight in knowing the detail which is coming next, his reaching out for his environment, his urge to create, his pleasure in his own skill.

One hesitates to compare scribbling to the half-automatic crying and babbling with which speech is inaugurated. The latter are certainly innate, and the organs exercised are essentially speech organs. Drawing is merely one of the innumerable accomplishments gained by the skilful control of the fingers.

The ordered course of linguistic development has been planned and plotted in numerous works. At first it would seem that this information already available would shed considerable light upon the present problem. Lukens attempted to show a complete parallelism.

In linguistic development the most important single element is the discovery by the child that everything has a name. In graphic development it would seem to be the first appreciation of resemblance or likeness—an interesting point in the light of Spearman's eductive principles.

The essential difference, rendering invalid any precise comparison, is the initiating motive. The young child uses speech to control and command. It is the medium for social expression.

No such fundamental need underlies the drawing act. It is true that many writers have maintained that the child of four years of age, when drawing, is really describing and communicating. It is possible on the other hand that the motives may be, when he is alone, those of self-amusement and self-assertion, and, when he is in the company of others, the showing-off of what he knows.

The most interesting of all the phenomena to be noted in the development of the child's drawing of a man is 'the mixed profile,' the spasmodic turning of details from the full-face to the profile representation. It appears with surprising regularity at the age of 6½ years, although some of the more recent American work would suggest that the deviation from 6½ is rather larger than is generally supposed. Thus McCarty gives the following percentages (based upon a very large number of cases):

Age ...	4 years		5 years		6 years		7 years		8 years	
	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls	Boys	Girls
Full-face representation	92	94	89	91	67	76	51	64	53	64
Profile representation	1	1	5	5	21	16	32	24	37	28

Doubtful drawings, including mixed profiles, account for the remainders.

Before discussing the significance of the change from the full-face representation to the profile, it must be pointed out that this applies only to the drawing of a man in a detached situation. In the illustrations of a theme the figures are quite frequently shown full-face and their representations are less detailed.

The occurrence of the mixed profile in the drawings of normal children happens at an interesting stage of their careers. Piaget, in his study of child logic, has called attention to the importance of the months with which the seventh year ends (6½–7). The appearance of the mixed profile coincides also with the passing of play as the entire attitude towards life. There is greater contact with reality. The desire for fact, it has been said, forces back imagination.

Previously children's drawings have been play figures. Much has been made of their symbolism. But does the child realize that they are symbolic? Various writers have pointed out the false logic of assuming that, because the child can indicate a picture and say 'pig,' he has learned anything more than that the sound 'pig' is associated with the picture. Are we quite certain that picture-language has always a more direct connection with the qualities of the object denoted than has sound-language?

Does the child who insists that some scribble is 'Mama' see any resemblance in his picture to his mother?—or does he perceive a similarity simply to a drawing of 'Mama' which someone has made for him?

Too much should not be made of this point. The schematic process is one of integration, and the child must learn during these acts of synthesis that a connection exists between the whole and its constitutive elements. Spatial arrangement forces him to perceive the relationship between the figure and its parts.

The child has first to learn, therefore, that the two small circles not only represent eyes, but resemble them. It is suggested that this process, which may be described, following Burt, as one of descriptive symbolism, holds until the appearance of the mixed profile.

Of all the symbolic components of the child's full-face drawing of a man, the nose bears least likeness to reality. The nose in profile is outstanding. It is easily drawn and its appearance is surprisingly realistic.

Steps underlying the first appearance of the second nose seem to me to include:

(*a*) The child's perception of a profile-drawn nose in pictures which he has seen.

(*b*) His apprehension of this detail as something not appearing in his drawing.

(*c*) Its tentative inclusion.

(*d*) The realization that this is a *nose* and that its inclusion is logically inconsistent with the presence of another nose.

(*e*) The appreciation of the greater realistic accuracy of the new representation.

During the next three years the child attempts descriptive (or logical) realism. Some details—such as the buttons—which are readily represented full-face may resist the change to profile.

More significant is, however, the representing of men in character during this stage. Also there are many theme-illustrations, and in these, as has been indicated above, it is the action of the man rather than the man himself which the child is endeavouring to represent. Not only are there fewer details, but the representation is frequently full-face.

The sequence followed in graphic development may be shown in tabular form:

General development	Graphic development	Characteristic features (following Burt)	Motivation*
Birth : 2½ (Physical adjustment)	2 : 4 (Scrawl)	Imitation : repetition	Self-amusement (Assertion of power)
2½ : 6–7 (Playtime of life)	4 : 6½–7 (Play)	Descriptive symbolism	Conventional play
7–8 (Transition stage)	7 : 9½ (Dawn of realism)	Logical realism	Expression to self
8–9 : 12–13 (Angular age)	9½ : 11–14 (Regression)	(Failure of) visual realism	—
13–14 : 16–17 (Puberty)	14–15 (? Revival)	—	—

* Also social approbation during all stages.

The desires underlying the attempts at objectification may be classified also. They are operative mainly during the stage of the dawn of realism.

Character of drawing	Objectified desire
Forms (letters, figures, flags, etc.)	Social approbation
Plants (trees, flowers, etc.)	Originally possession (social approbation)
Animals (birds, insects)	Possession or presence (many are mere forms, *e.g.* the kindergarten cat)
Ships and mechanisms (human beings generally included)	Possession or fanciful self-assertion. (Social approbation plays a large part, for example, in the continual reproduction of ships)
Houses	Presence or fanciful self-assertion
Human beings	Play, and, possibly, some self-assertion
Scenes	Presence or recall
Human beings (characters)	Fanciful self-assertion or recall (become very mechanical with praise)
Illustrations	Recall (? largely motivated by social approbation)

An interest in things.

Thus to sum up the arguments given above, three fundamental factors enter into the drawing process:

(*a*) The ability to analyse and synthesize.

(*b*) The desire to apply this ability to a particular subject or group of subjects for the purposes of objectifying his desires. (Burt has suggested that this factor comprises (1) an interest in the visual appearance of things, and (2) an interest in reproducing these appearances.)

(*c*) The fineness of the quality of hand-eye co-ordination.

Of these the least important is (*c*). It is popularly supposed that the artist possesses exceptional kinaesthetic control, and that a comparative clumsiness limits his more normal fellows. That the artist is unusually skilful is beyond question, but practice and training play their part. Awkwardness will certainly discourage a child when he realizes that he is awkward. But it is a doubtful point if most children realize for themselves their deficiencies before they have been pointed out by the teacher and others. Self-criticism is not in evidence before adolescence (but consciousness of failure to win approval most certainly is).

The factor (*a*) is closely correlated in my opinion with intelligence. I have suggested that it is not affected by individual variations in the qualities of *innate* visual powers. If this assumption is incorrect, the argument falls.

The determining factor is (*b*). Thorndike and others have divided mankind into those who work with ideas and those who work with things—the word (or abstract) thinkers and the thing (or concrete) thinkers. Experimental evidence on this point is not encouraging, but that some such distinction does exist is assumed in this article.

Now drawing may be defined as the act of representing objects by means of lines. During, at the least, childhood, it is essentially concrete.

One is attracted by the hypothesis that the factors underlying success at drawing are (*a*) intelligence, (*b*) the desire to think concretely.

Burt has suggested a possible subdivision of the concrete thinkers into those who handle the realities (the mechanics) and those who think in terms of them (the draughtsmen and the artists). The trend of the day, with its ready supply of constructional toys, is probably towards the increase of the first group at the expense of the second. It may be pointed out that the young builder does seem to draw his motor cars and his ships fairly frequently and that they are generally peopled, so that the choice of a man as subject tends to bring the two concrete groups together. But the whole discussion must await experimental evidence.

X. THE MOST SUITABLE SUBJECT FOR DRAWING RECONSIDERED.

Developing the suggestion that the division between these two psychological types is fundamental, we can proceed to distinguish six drawing types:

Intellectually	above normal	Word-thinker
,,	,,	Thing-thinker
,,	normal	Word-thinker
,,	,,	Thing-thinker
,,	below normal	Word-thinker (? non-existent)
,,	,,	Thing-thinker

At an earlier stage it was maintained that an intelligence test based upon the drawing of one subject (a man) was inadequate. Now, however, I suggest that the test should not be used for measuring intelligence. Rather should we begin possessing the child's I.Q., and endeavour to classify him as either a word-thinker or a thing-thinker.

The former, unlike the thing-thinker, will not score well. Therefore, to increase our chances of detecting him, we shall make our test as concrete as possible. A man is the ideal subject (or alternatively a theme with a man as central figure, *e.g.* "a gentleman and his little dog walking down the street: it is raining, so he has his umbrella up"). It has the added advantage of being remote from the influences of scholastic training. The schema is the product of integration. Practice (repetition) is essential. If I am justified in suggesting that the child who draws often cannot help but include men frequently, then, as a result, his schema of a man will be more fully developed.

The word-thinker of marked intellectual ability will do fairly well in the test. Unable to base his drawing upon an extensive schema, his general intellectual ability will enable him to summon much associated detail to his assistance. Thus one finds in these drawings unexpected features—unusual points scored, obvious points omitted.

XI. THREE SUGGESTED SCALES.

Goodenough includes over fifty points in her scale. But of these many are scored by no young children, and others are scored by all older children. The examining of drawings for these points is, therefore, rather laborious and monotonous. It is suggested that an improvement would be effected by drafting three scales with the appropriate points for (*a*) the pre-mixed-profile years (2 : 6½–7); (*b*) the pre-regression years (6½–7 : 9½–10); (*c*) early adolescence (9½–10 : 14–15). Each scale may be reduced to twenty points. Three drawings made on separate days or weeks should be examined.

The first scale (2 : 6½–7). The motive during these early years being assertion of power rather than expression, it follows that the drawing scores should correlate closely with intelligence. It will be noticed, however, that the correlation coefficients given by McCarty and Goodenough have little in common. Unfortunately the factors set out below as affecting success at the test are very marked at this stage. I suggest that we cannot apply a scale to the drawings of a child who has been in school less than three months and expect to obtain accurate measurement.

The second scale ($6\frac{1}{2}$–7 : $9\frac{1}{2}$–10) is in my opinion the most useful of the three scales.

The third scale ($9\frac{1}{2}$–10 : 14–15). Unfortunately regression influences these drawings, but they should be of value in enabling us to devise a coefficient of regression throwing considerable light on individual problems of adolescence. Details worthy of special consideration in this respect are the treatment of hands, arms, difficult articles of clothing, general proportions, and lack of decisiveness in line.

XII. SOME FACTORS AFFECTING SUCCESS AT THE TESTS.

(1) Environmental conditions. The child learns to play the drawing game from others, and his skill is determined by their skill. His desire to play the game originates, to some extent, in their willingness to play it with him.

(2) The supply of pictures. Too much importance can be attached to this point which is fully discussed by Stern.

(3) The companionship of other children.

(4) Training in the use of a pencil (crayon work cannot be accepted).

(5) The influence of formal instruction which may have been given in the drawing of a man.

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MEMORY AND MENTAL IMAGERY.

AN EXPERIMENTAL STUDY[1].

By HENRY BOWERS.

(*High School, Fergus, Ontario.*)

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II. *The problem* (p. 272).
III. *Outline of method and conclusions* (pp. 272–273).
IV. *General plan of experiment* (pp. 273–276).
V. *Consideration of a series of correlations* (pp. 276–281).
VI. *Remarks on method* (p. 281).
VII. *Conclusions* (pp. 281–282).

I. INTRODUCTION.

No attempt will be made to review at length the extensive literature dealing with memory and imagery. Such questions as the influence on degree of retention of mode of presentation and type of material have been the subjects of much experimental study. In this connection the bearing of ideational type naturally came under consideration. However, criticism has been directed against the tendency to infer imaginal type from ability in remembering given material. This criticism is supported by the finding of Foster (3) that the work of observing visual material might be transferred to another sense department, and that such transfer might even be advantageous.

In a recent article (2) the writer drew attention to the existence of a relatively constant order of precedence in certain words isolated from context, with regard to visual imagery aroused by them in a group of secondary school pupils. (A similar condition was found to exist when words denoting sound were used.) Pupils were asked to rate their imagery as 3, 2, 1, 0, for each word, according as they experienced images ranging from very clear and distinct, through medium clarity to no image. The 'mean image' aroused by each *word* was calculated. These 'mean images' obtained from one group of subjects were correlated with those secured

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[1] The writer wishes to acknowledge gratefully the assistance rendered by Messrs A. and S. Holmes in administering several of the tests. Prof. Sandiford and Prof. Bott of the University of Toronto have most readily given valuable advice.

from another, yielding the Pearson *r* coefficients 0·86 ± 0·04 and 0·86 ± 0·03. From data secured in the present study the comparatively high coefficient 0·94 ± 0·01 was calculated.

II. The problem.

It seemed that this establishment of a fairly constant order of precedence of certain words in respect of the imagery aroused by them in a given group of subjects afforded a new approach to the problem of influence of imagery on memory. Data were secured from which coefficients of correlation were calculated between images for 'visual,' 'auditory' and 'kinaesthetic' words claimed by a group of subjects and the numbers of times these words were recalled by the group.

III. Outline of method and conclusions.

A list of 143 monosyllabic words whose concomitant imagery was likely to be visual was made up by taking three selections of the same forty-five words, each selection being in a different chance order. Four 'dummy' words were placed at the beginning and four at the end of the list. These 'buffers' were to obviate the effects of primacy and recency, and played no further part.

This list was read to groups of subjects in accordance with an objective technique previously developed and practised. Reproduction of the words in any order was then required.

'Auditory' and 'kinaesthetic' words were also employed.

After the reproduced lists were collected a brief talk on the relevant type of imagery was conducted. The subjects were then requested to rate the imagery aroused by each word on a scale of 4 (2).

Thus, for each word were known the 'mean image' claimed by the group, and also the number of times it was remembered. From these figures coefficients of correlation were calculated.

The rate at which the words were read was varied, and also the time elapsing before reproduction was requested in an attempt to discover optimum conditions for the functioning of imagery.

It was found that:

(1) A small but significant positive correlation existed between the numbers of times visual and kinaesthetic words were remembered and the imagery claimed for them.

(2) In the case of auditory words the corresponding coefficient was zero.

(3) Decrease of association between the words was without appreciable effect on the coefficient.

(4) The coefficient for words in the first third of the reproduction list was no higher than that for words in the last third.

(5) The relative constancy of imaginal content previously found for visual and auditory words was confirmed.

(6) A similar property existed in the case of kinaesthetic terms.

IV. GENERAL PLAN OF EXPERIMENT.

The general plan adopted was as follows. Three lists, each containing forty-five monosyllabic words, were formed, one of visual, another of auditory, and the third of kinaesthetic terms. From each of these lists a new one, containing 135 words was made, by a random selection of the forty-five words for its first third, a second random selection for its next third, and similarly for the last third. To this new list eight 'dummy' words were added—four at the beginning, four at the end. This effectually counteracted the effect of primacy and recency on memory scores for the forty-five experimental words. These 'dummies' had no further place in the experiment. The list of 143 words was read to a high or public school class, at a fixed rate—either 1 or 2 seconds per word, and in accordance with an objective technique, developed and practised in preliminary experiments. This procedure is not published here, but will be gladly supplied by the writer to any worker who wishes to repeat the experiments. It may be mentioned that the mean time error in reading is estimated at 3 per cent. To obviate as far as possible any tendency to speaking movements on the pupils' part, they were required to hold their tongues against their front teeth. In some cases the subjects wrote the words in any order immediately on completion of the reading, in others a 2-minute period intervened. This interval was employed in performing multiplication. The pupils were given all the time they required. When everyone had finished a brief talk was conducted on the appropriate type of imagery (see "Constancy of Imaginal Content"). Then sheets were distributed containing the word list and instructions for rating imagery aroused by each word. (The fact that this list had been read should be remembered when considering scores made after a 30-day interval.) Special warnings were given in connection with the estimation of kinaesthetic imagery to avoid imitative body movements, also not to confuse this imagery with mental pictures of people performing the various movements.

It seemed not unreasonable to suppose that the correlation between imagery for words and word score would be influenced by the time allotted to each word when reading the list. A longer period would presumably allow more opportunity for the arousal of images. There was also the possibility that the time-interval elapsing between reading and reproduction might have some bearing on the results.

Reference to Table I will make clear the several variations in procedure.

The following are the lists used, placed in alphabetical order:

Visual: beard, boat, clan, dime, elf, fiend, ghost, grass, hat, hen, jug, path, pew, phrase, plank, porch, pouch, prince, purse, quill, rib, rogue, rug, rust, scar, shrub, sky, slum, snake, swan, tent, thief, toad, tongs, tweed, twin, valve, van, verb, wad, wand, wart, weed, wig, wisp.

Auditory: bang, blare, bleat, buzz, call, caw, chime, chirp, clang, clap, click, cluck, coo, crash, crunch, cry, groan, growl, grunt, hiss, hoot, hum, mew, moan, moo, pop, purr, quack, roar, scold, scream, shout, shriek, shrill, slam, snore, splash, squeak, squeal, thud, toot, whine, yap, yell, yelp.

Kinaesthetic: bite, bump, chew, clasp, climb, cling, dance, dig, fall, fling, glide, grasp, grip, heave, hit, hop, hurl, jump, kick, leap, prod, pull, punch, push, ride, rub, run, scratch, scrub, shake, shove, shrug, slide, slip, speed, squeeze, strike, sweep, tear, tug, throw, twinge, twist, twitch, walk.

Circumlocution will be avoided by the use of the following contractions: (*a*) 'mean imagery for words,' to imply the mean imagery rating obtained from a group of subjects for each word; (*b*) 'imagery for subject,' for the total imagery rating of each subject on a given list of words; (*c*) 'word score,' for the number of times a given *word* was recalled by a group; (*d*) 'subject's memory score,' for the total score of a given subject in reproducing a particular word list.

In this article the following will be considered—the correlation between mean imagery for words and

(1) Word score.

(2) Word score, after the elimination of common associates.

(3) Word score, in the first one-third of each subject's reproduction list.

(4) Word score, in the last one-third of each subject's reproduction list.

(5) Word score, after the lapse of 30 days.

The following coefficients were calculated incidentally between:

(1) Imagery for subjects and subjects' memory score.

(2) Visual and auditory imagery for subjects.

(3) Auditory and kinaesthetic imagery for subjects.

(4) Kinaesthetic and visual imagery for subjects.

(5) Word scores obtained from two groups of subjects to whom words of the same kind (visual, auditory or kinaesthetic) had been given.

Table I. *Coefficients of correlation between imagery for words and (a) word score, (b) word score after partial elimination of associates, (c) word score for first third of reproduction list, (d) word score for last third of reproduction list, (e) word score after 30-day interval.*

					Pearson r coefficient of correlation between words and				
Type of words	Time per word in reading (sec.)	Reproduction (min.)	Grade	No. of subjects	Word score	Word score after partial elimination of associates	Word score for first third of reproduction list	Word score for last third of reproduction list	Word score after 30-day interval
Visual ...	2	Immediate	1st yr. H.S.*	31	·25 ± ·09	·29 ± ·09	·15 ± ·10	·08 ± ·10	·10 ± ·10
Visual ...	1	”	2nd yr. H.S.	36	·33 ± ·09	·28 ± ·09	·16 ± ·10	·18 ± ·10	·35 ± ·09
Auditory ...	2	”	1st yr. H.S.	38	·09 ± ·10	·07 ± ·10	·12 ± ·10	·04 ± ·10	·38 ± ·09
Auditory ...	2	After 2	Final yr. P.S.†	33	·11 ± ·10	·07 ± ·10	·01 ± ·10	·16 ± ·10	—
Auditory ...	1	After 2	Final yr. P.S.	34	·08 ± ·10	·08 ± ·10	·13 ± ·10	·04 ± ·10	—
Kinaesthetic	2	Immediate	1st yr. H.S.	39	·12 ± ·10	·11 ± ·10	·02 ± ·10	·26 ± ·09	·36 ± ·09
Kinaesthetic	2	After 2	Commercial (from 1st, 2nd and 3rd yr. H.S.)	30	·38 ± ·09	·35 ± ·09	·34 ± ·09	·36 ± ·09	·48 ± ·08
Kinaesthetic	1	After 2	3rd and 4th yr. H.S.	35	·24 ± ·09	·21 ± ·09	·14 ± ·10	·27 ± ·09	·39 ± ·08
Kinaesthetic	2	Immediate	2nd yr. H.S.	37	·39 ± ·08	—	—	—	·36 ± ·09

* High (secondary) school.

† Public (primary) school.

(6) Subjects' memory scores on visual and auditory terms, etc.

(7) Imagery rating for visual (auditory and kinaesthetic) words obtained from one group and that obtained from another.

V. Consideration of a series of correlations.

Correlations between mean imagery for words and word score.

Pearson *r* coefficients between mean imagery for words and word score are reported in Table I.

In the case of visual words the coefficients are small but positive. In view of the size of the probable error, it may be said that there is no appreciable alteration of the coefficient effected by changing the time from 1 to 2 seconds in reading visual words. Previous experiments, in which a rather different technique was employed, indicate that delay before reproduction exerts negligible influence on the coefficient. The average of four coefficients, when immediate reproduction was asked, was 0·27 ± 0·12, and that of four coefficients obtained with delayed reproduction was 0·18 ± 0·12.

Coefficients found with auditory words were much smaller. Taking into account the size of the probable error, the positive sign of all three may be merely a coincidence. Again, the effect of varying the time of reading, or the interval before reproduction was insignificant. The coefficients for kinaesthetic words were higher, averaging almost the same as those for visual terms. There is no apparent influence on the coefficient of variation of reading time or interval before reproduction.

Correlations between imagery for subjects and subjects' memory score.

It might be of interest, at this point, to record correlations between the sum of imagery ratings of pupils with their memory score. Since there could be no basis of comparison between the sum of the imagery ratings of one pupil on a given word list with that of another, zero correlation might be expected. This was the case. For visual words Pearson's *r* was – 0·09 ± 0·11, for auditory words 0·11 ± 0·11, for kinaesthetic terms – 0·11 ± 0·11. Thirty-four subjects were used in each correlation.

Correlations between mean imagery for words and word score after elimination of common associates.

One could not help being impressed by the frequency with which certain words were found paired together in the reproduction list. It would appear that the phrase 'words isolated from context' requires modification, for there is evidently a striving after artificial context when a natural one is not provided. Association seems to be a very potent factor in the recall of words 'isolated from context.'

Since associations are frequently made for reasons obviously unconnected with the imagery under investigation (*e.g.* 'red—wed'), it seemed possible that the diminution of association might render more apparent any connection between imagery and retention. Such decrease might be expected to result in an increase in *r*.

For the purpose of this study the writer makes a convenient division of association into 'common' and 'private.' Common association is word-linkage characteristic of the group, and due doubtless to the influence of a more or less similar environment. This type is detected when words pair together a number of times, considerably exceeding that expected on a chance basis. Private association is word-linkage peculiar to the individual and cannot be detected objectively. Even introspection will frequently be at fault in uncovering the more subtle private associations. The only distinction which concerns us here is the difference in methods of detection.

The following example will illustrate the device adopted for the detection and partial elimination of common association. Suppose the original list consisted of the following words: boat, arm, desk, red, man, tree, book, toad, snake, snow. These were arranged vertically and horizontally as indicated.

	—	Arm	Boat	Book	Desk	Man	Red	Snake	Snow	Toad	Tree
—	.	.	.	.	.	.	1	.	.	.	.
Arm	.	.	.	.	.	.	.	.	.	.	.
Boat	1	.	.	.	.	.	.	.	.	.	.
Book	.	.	.	.	.	.	.	.	.	.	.
Desk	.	.	.	.	.	.	.	.	.	.	.
Man	.	.	.	.	.	.	.	.	.	1	.
Red	.	.	.	.	.	1	.	.	.	.	.
Snake	.	.	.	.	.	.	.	.	.	.	.
Snow	.	.	1	.	.	.	.	.	.	.	.
Toad	.	.	.	.	.	.	.	.	1	.	.
Tree	.	.	.	.	.	.	.	.	.	.	.

Then, a reproduction list such as follows: red, man, toad, snow, boat, would be read: blank red; red man; man toad; toad snow; snow boat; boat blank; and entered in the proper square. It will be noted that concentration of any word at the beginning or end of the list can be detected. Each combination was considered a separate association, thus 'red-man' was considered separately from 'man-red.' The number of entries that might be expected in each square was calculated. It varied from 0·11 to 0·21. Accordingly, common association was held to exist whenever two or more entries occurred in any square. Since one entry necessarily could not indicate common association, it appeared fair to exclude only the excess of entries over one in such a square. Corrected totals were then found.

The writer is fully alive to the arbitrary nature of some of his assumptions, also to the fact that conclusions regarding the presence of association drawn from a comparatively small total number of entries must be only approximate. Yet, it should be remembered, that his object was to determine the effect on correlation of *diminishing* association.

Coefficients of correlation between imagery for words and word score are given in Table I. It will be observed that there is no marked tendency to increase over or decrease from the coefficient already discussed.

It may be asked: "Why should such associations as 'crash-bang,' 'slam-bang,' 'buzz-hum,' be eliminated? May not the association of these words be occasioned by arousal of imagery?" This is admitted, but the aim was to study retention of words isolated from context, or even, as far as possible, from artificial context. The serial nature of the words is an unfortunate circumstance, and diminution of the possible effects of this was the object of this part of the work.

Correlations between imagery for words and word scores in the first third and also in the last third of reproduction lists.

Betts (1) concludes that imagery is especially excited when an obstacle in thinking is encountered. This suggested the possibility that scores for words occurring at the end of the reproduction list might correlate more highly in the imagery than those for words at the beginning. Some support for such a possibility is afforded by the apparent marked increase in effort necessary for recall of those last written.

The coefficients given in Table I are no larger for the last third than for the first.

Correlations between imagery for words and word score after the lapse of 30 *days.*

After a 30-day interval those classes that were available were requested to write as many as possible of the words. The resulting scores were, of course, influenced not only by the writing of the words in the previous test, but also by the reading of them from the mimeographed sheets when imagery ratings were being made. Table I contains the coefficients calculated.

Contrasting these coefficients with those obtained from the original memory scores a small decrease is noted in the case of visual words, a substantial increase with auditory words, and a less marked, but nevertheless significant, advance with kinaesthetic words. It might be mentioned that in previous experiments an increase from 0·10 to 0·17 was noted after 20 days, using visual words. However, in view of the large probable errors it is best to await further evidence before attempting to draw definite conclusions.

Correlations between subjects' memory score on words of each type.

The figures obtained made possible the calculation of certain other coefficients, which, without direct bearing on the problem, are interesting enough.

Total scores for pupils on visual, auditory and kinaesthetic terms were employed. These coefficients were calculated, between:

Subjects' scores on visual and auditory words	·35 ± ·10
Subjects' scores on auditory and kinaesthetic words ...	·53 ± ·09
Subjects' scores on kinaesthetic and visual words	·51 ± ·09
Subjects' scores on kinaesthetic and visual words	·51 ± ·09

Correlations between word scores.

Coefficients between word scores obtained from one class and those from another were calculated:

Visual word scores ...	·56 ± ·06
Auditory word scores ...	·68 ± ·05; ·70 ± ·05; ·62 ± ·06
Kinaesthetic word scores	·57 ± ·07; ·64 ± ·06; ·74 ± ·05; ·63 ± ·06; ·74 ± ·05; ·63 ± ·06

It is to be noted that coefficients from word scores are markedly higher than those from subjects' scores. Just as there appears to be a relatively constant order of precedence in words in respect of the mean imagery attached to them by a given group, so there would seem also to be an order of memorial precedence. This property is not so marked in the latter as in the former. Whipple ((5), p. 201) remarks: "In word tests, certain terms are often found to have a special reproducibility, evidently by attracting attention in some way. Thus Binet and Henri found that the word *pupitre* (desk), though in the middle of a series, and hence unfavourably placed, was recalled in an unusually large number of cases."

The question as to the effect of partial elimination of associates on order of precedence in respect of memory might be raised. Earlier work of the writer points unquestionably to a decrease of the coefficient. The following pairs of coefficients are those for 'memorial content' before and after elimination of common associates: 0·50, 0·28; 0·80, – 0·05; 0·60, 0·54; 0·67, 0·58; or, taking the means: 0·64, 0·33. Such decrease indicates that the relatively high coefficients between word scores reported above may, in part at least, be due to common environmental influences.

It should be borne in mind that the word scores for visual, auditory, and kinaesthetic words were obtained under different circumstances. For example, the coefficient of 0·56 ± 0·06 given above for visual words was obtained by using scores from two classes in which a 2-second and a 1-second reading interval were respective experimental variations.

Correlations between subjects' imagery for different kinds of words.

Imagery ratings having been obtained for one class for visual, auditory, and kinaesthetic words, it was possible to calculate coefficients between subjects' ratings for the three pairs.

Visual and auditory imagery ratings	·73 ± ·05
Auditory and kinaesthetic imagery ratings ...	·74 ± ·05
Kinaesthetic and visual imagery ratings... ...	·77 ± ·05

Coefficients obtained in previous work, using visual and auditory words only, were higher, viz. 0·80, 0·86, 0·76, 0·84.

High coefficients are reported by Betts (1) for similar measures.

It is submitted that at present no interpretation of these coefficients is legitimate, other than that they are due to consistent tendency of each subject to rate high, medium or low.

Correlations between mean imagery ratings for visual (auditory or kinaesthetic) words obtained from two groups.

No coefficients for kinaesthetic images were reported in "Constancy of Imaginal Content." Data secured in this study supply that deficiency and confirm those reported for visual and auditory words. These coefficients were calculated:

Mean imagery for visual words	·94 ± ·01
Mean imagery for auditory words	·87 ± ·02; ·84 ± ·03; ·86 ± ·03
Mean imagery for kinaesthetic words ...	·88 ± ·02; ·92 ± ·02; ·88 ± ·02; ·88 ± ·02; ·93 ± ·02; ·83 ± ·02

Mean memory scores for subjects.

Table II shows the mean scores made by pupils of two classes on different kinds of terms.

The order for memory for these words is: auditory, kinaesthetic, visual, both for immediate reproduction and after 30 days.

Table II. *Mean memory scores* (*immediate reproduction and after* 30 *days*).

Class	Reading interval (sec.)	Reproduction	Mean score			Mean score after 30 days		
			Vis.	Aud.	Kin.	Vis.	Aud.	Kin.
1st yr. H.S.	2	Immediate	9·10	14·70	11·53	1·10	4·50	2·30
2nd yr. H.S.	1	„	14·44	—	17·30	3·07	—	6·64

VI. REMARKS ON METHOD.

1. It may be objected that the groups employed are too small. An answer to this is provided by the coefficients obtained from the mean imagery ratings, and also from the word scores. For the former these were 0·94, 0·87, 0·84, 0·86, 0·88, 0·92, 0·88, 0·88, 0·93, 0·83. For the latter the figures were 0·56, 0·68, 0·70, 0·62, 0·57, 0·64, 0·74, 0·63, 0·74, 0·74, 0·74, 0·63. Surely a smoothing-out of individual variations is indicated not only by the magnitude of these r's, but also by their relative constancy. It is hardly necessary to point out that the P.E. depends not on the number of subjects, but on the number of words. An increase in the latter would of course entail a compensating increase in the former.

2. An assumption underlying the study is that coefficients of correlation involving memory and imagery are comparable when obtained from forms of differing standards in high and public schools. Scrutiny of the figures in Table I lends a considerable degree of support to this hypothesis.

VII. CONCLUSIONS.

1. There is a small but significant positive correlation between word scores and mean imagery for words in the case of visual and kinaesthetic terms.

2. If auditory words be employed the coefficient is zero.

3. Partial elimination of associates from the list of recalled words has no appreciable effect on the correlations.

4. Scores for words found in the last third of the recalled list yield no higher coefficients with imagery than those in the first third. Assuming that the last words written are recalled with greater effort than the first, it would appear, either, that this effort does not consist in appeal to imagery or that, as Moore (4) suggests, it is recognition of the image rather than its distinctness that counts. Even in the latter event it is difficult to comprehend why recognition is not aided by clarity and impeded by obscurity.

5. The lapse of a month appears to result in a higher correlation between imagery for words and word scores in the case of auditory and kinaesthetic terms. The evidence is rather contradictory when visual words are considered.

6. As might be expected, total imagery ratings for pupils have zero correlation with pupil memory scores.

7. Marked correlation between pupil memory scores is reported.

8. Still higher correlations were obtained between word scores from two groups of subjects for words of the same kind.

9. There is evidence that, for visual terms, the partial elimination of association results in diminution of the coefficients referred to in (8) above.

10. The figures reported in "Constancy of Imaginal Content" are confirmed for visual and auditory terms. A similar condition was found to apply in regard to kinaesthetic terms.

11. Both for immediate reproduction and after a 1-month lapse the order for recall of the different kinds of words used was auditory, kinaesthetic and visual.

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ON THE SATURATIONS AND CHROMATIC THRESHOLDS OF THE SPECTRAL COLOURS[1].

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I. *Introduction* (pp. 283–289).
II. *Description of apparatus* (pp. 290–292).
III. *The colourless interval* (pp. 292–301).
IV. *The intensity required for maximum saturation* (pp. 302–304).
V. *The chromatic threshold with respect to white* (pp. 304–312).
VI. *Summary* (pp. 312–313).

I. INTRODUCTION.

WORKERS in the domain of colour vision have generally been concerned with the problems relating to the perception of hue, to the neglect of another, less impressive aspect of visual sensations, namely, saturation. No theory of colour vision, however, can be regarded as adequate if it does not account for the laws of saturation as well as for those of hue. Our knowledge of the former laws is unfortunately rather meagre. Experimental work in this field might be expected to lead to a broader and more inclusive conception of the visual mechanism than we possess in the present theories, which are based upon a more restricted group of facts. One of the most fundamental problems of the psychophysiology of vision is that of the relationship between the chromatic and achromatic processes; this is one of the main points, for example, upon which Helmholtz and Hering disagreed. A careful study of the phenomena of colour saturation might be expected to throw light upon this important question.

The word 'saturation,' according to the prevailing usage, stands for a strictly subjective aspect of visual experience. Sensations of vision, it is generally agreed, have three attributes, namely brilliance[2], hue and saturation. All of these attributes are *defined* in psychological, not in physical or physiological terms. They may each have a certain correlation with aspects of the physical stimulus and with aspects of the physiological response, but this correlation is a matter of empirical proof or of assumption, and is not involved in the definition of the attribute in question.

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[1] The writer is grateful to Dr L. T. Troland for advice and criticism, as well as for the opportunity to use the latter's colour-vision apparatus in this research.

[2] The term 'brightness' is undesirable in this connection, on account of the established usage of the same word in a different sense in photometry. Cf. L. T. Troland, "Report of the Colourimetry Committee of the Optical Society of America," *J. Opt. Soc. Amer.* 1922, VI, 531.

The definition of saturation which has been adopted by the Colourimetry Committee of the Optical Society of America, and to which I shall adhere in this paper, is the following: "Saturation is that attribute of all colours possessing a hue, which determines their degree of difference from a grey of the same brilliance[1]." This is a strictly psychological definition.

A good deal of confusion has arisen through the practice of Helmholtz and other physicists who employ the word 'saturation' not in the sense of an attribute of sensation but as a description of the physical stimulus, or of the hypothetical physiological process which gives rise to sensation. Thus the word 'saturation' is frequently used by Helmholtz and his followers to signify the relative amount of achromatic component in the retinal response, as inferred not from direct inspection, but from the laws of trichromatic mixture. At times, however, Helmholtz used the same word in a different sense, *e.g.* in speaking of the 'saturation' of a contrast colour or of an after-image. It is obvious that if we choose to define saturation in some sense other than the psychological, we require some different word to describe the psychological characteristic of sensation.

It may be said that this distinction is unimportant, on the ground that Helmholtzian saturation and psychological saturation vary in strict parallelism with each other. But such a parallelism cannot be taken for granted until it has been experimentally established, and up to the present time the requisite experimental proof has never been performed[2].

The comparison of saturations, in our sense, is a very difficult matter, except under one special condition, namely when the colours to be compared have the same hue and brilliance. Tschermak goes so far as to say: "Die Frage, ob die verschiedenfarbigen...Eindrücke spektraler Lichter *dieselbe* Sättigung besitzen oder nicht, ist schwer zu beantworten[1]." But I believe that most observers will agree with me in finding no difficulty in seeing that the spectrum is not equally saturated throughout, and in particular that the red and violet are decidedly more saturated than the yellow. In my own case I am able to say that the red (wave-length greater than $670 m\mu$) is clearly more saturated than any other spectral hue of equal brilliance, at any rate when the intensity is not above 200 photons.

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[1] Troland, *op. cit.* p. 534.

[2] There is a variety of possible ways of defining Helmholtzian saturation, and these lead to as many different quantitative results; cf. I. Runge, "Die Einheitsmengen in Maxwell-Helmholtzschen Farbendreieck und die Bestimmung der Farbsättigung," *Zschr. f. Instrumentenkunde,* 1929, XLIX, 600. According to the simplest and most customary definition, saturation is the ratio of achromatic to chromatic excitation. The experiments of W. Seitz ("Über die Definition der Sättigung einer Farbe nach Helmholtz und Exner und über das Ostwaldsche Farbensystem," *Zschr. f. Sinnesphysiol.* 1923, LIV, 146), indicate that saturation as thus defined is in pronounced disagreement with psychological saturation.

In the present study I have not attempted the awkward task of directly comparing the saturations of colours of unlike hue and brilliance, but have confined myself to certain problems which are more amenable to experimental attack. The first of these problems is that of the variation of saturation with the intensity of the stimulus.

As the intensity of a chromatic stimulus is increased, the experienced colour undergoes characteristic changes in saturation. At very low intensities, under ordinary conditions of vision (where both rods and cones function), all sensations are completely de-saturated, and the spectrum is simply a band of grey. As the intensity is increased, hues begin to appear. There is a definite 'colourless interval' between the 'general threshold,' or the intensity which just suffices to produce a sensation of light, and the 'specific threshold,' or the intensity at which colour is just noticeable[2].

The saturation of the colours steadily increases with rising intensity until a certain level is reached at which the saturation is a maximum. Further increase of intensity results in a diminution of saturation, and, according to the generally accepted notion, if the intensity is made sufficiently high the colours once more become completely de-saturated, and the entire spectrum appears white.

It is held by some authorities (*e.g.* König and Nagel) that the colourless interval is strictly a phenomenon of scotopic vision. The rods function at an intensity which is too low to excite the cones and yield an achromatic sensation. One would expect then that when the stimulus is confined to the central rod-free area of the retina, no colourless interval will exist. König, Nagel and von Kries maintain that this is in fact the case when spectral colours are observed. But other experimenters have obtained contrary results, and the question is still controversial[1].

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[1] A. Tschermak, "Licht- und Farbensinn," *Bethes Handb. d. Physiol.* Berlin, 1929, XII (1), 366.

[2] I am using von Kries's terminology. Some writers use the terms 'photochromatic interval,' 'absolute threshold,' and 'chromatic threshold.' The use of 'chromatic threshold' in the present connection is avoided on account of a possibility of confusion with another sort of chromatic threshold which is to be discussed later.

The recent careful experiments of Miss Monroe[2], in which a field of 2° 2·2′ was employed, point to the existence of a definite foveal colourless interval for the spectral colours. The longest wave-length she employed was $655 m\mu$. Some experimenters have reported that the colourless interval is absent for wave-lengths greater than this value, in the red end of the spectrum.

According to Burch[3], the existence of a foveal colourless interval under ordinary conditions of observation is due to the presence of 'dazzle-tints'—positive after-effects of stimulation which take a long time to subside. When dazzle-tints are eliminated by resting the eye in darkness for two hours, stimuli appear coloured even at the threshold of vision.

Abney and Watson[4], in 1916, performed some interesting experiments which would suggest a reconciliation of the conflicting statements of different experimenters with regard to the central colourless interval. These writers maintain that there is one class of observers who perceive such an interval, and another who do not. Now, when the same observers are subjected to other tests, it is found that for the first class there is lacking that sharp demarcation between the phenomena of peripheral and central, or rod and cone vision, which exists for the second class. Abney and Watson draw the conclusion that the centre of the retina is not invariably a rod-free region, but that some persons possess a quite appreciable proportion of foveal rods. For these subjects, foveal stimuli of threshold intensity excite only the rods, and hence appear colourless. The foveal colourless interval, like the peripheral, is a phenomenon of scotopic vision.

The spectral colours lose their saturation not only when the intensity is very low, but also when it is very high. It is generally supposed that at some sufficiently high intensity this loss of saturation is complete for all hues, although this supposition does not seem to have been rigorously verified by experiment. Observation with exceedingly intense stimuli is difficult, and the results are complicated by the action of chromatic fatigue, which becomes more rapid and more pronounced as the intensity is increased. One author, indeed, believes that fatigue is responsible for the whiteness of the high-intensity spectrum, and that the apparent whiteness in the green region, for instance, is only "a transitory stage in the production of green-blindness[1]."

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[1] Cf. Sir J. H. Parsons, *Introduction to the Study of Colour Vision*, 2nd ed. (Cambridge, 1924), pp. 66 ff.

[2] M. M. Monroe, "The energy value of the minimum visible chromatic and achromatic for the different wave-lengths of the spectrum," *Psychol. Monog.* No. 158, 1925.

[3] G. J. Burch, "On colour vision by very weak light," *Proc. Roy. Soc.* B, 1905, LXXVI, 199.

[4] Sir W. de W. Abney and W. Watson, "The threshold of vision for different coloured lights," *Phil. Trans. Roy. Soc.* A, 1916, CCXVI, 91.

Even though fatigue be responsible for the ultimate whiteness of the spectral colours, it cannot be the sole cause of their decline in saturation. For, as we shall see later, this decline begins at very moderate intensities, where fatigue acts in relatively mild and slow fashion.

Many writers attach considerable importance to the question whether colours become completely achromatic at the two extremes of the intensity scale. For example, Burch thinks he has refuted Hering's theory by showing that the de-saturation is not, in fact, complete in the case of the spectral colours. Nagel apparently feels that an important difficulty for the duplicity theory is removed if it can be shown that the spectral hues display no colourless interval in the fovea.

On the other hand, no one denies that the spectral colours have very low saturation when the light is weak, and also when it is glaringly bright. It is universally agreed that saturation increases with intensity up to a certain point where it reaches a maximum, and then declines as the intensity is raised beyond this point. The present writer believes that this general behaviour of colours presents the really significant problem here, and that the question of the limiting values of the de-saturation effect is by no means one of fundamental importance. Even if there be no complete de-saturation, the problem of explaining a partial de-saturation is essentially the same.

Furthermore, it should not be thought that the behaviour of the spectral colours in this respect is of all-important significance. Although there has been disagreement about the existence of a foveal colourless interval for the colours of the spectrum, it is an undisputed fact that such a phenomenon exists for ordinary unsaturated colours (*e.g.* pigment colours). There is no reason to suppose that spectral colours must be used for a crucial test.

Let us consider the interpretation of the saturation changes which is offered by Helmholtz. According to his theory, the white sensation corresponds to equal amounts of the elementary 'red,' 'green' and 'blue' processes. A colour is saturated in proportion to the degree of predominance of the two strongest excitations over the weakest excitation, that is to say, in inverse proportion to the quantity of white which it contains. Helmholtz[1] accounts for the de-saturation of colours at high intensities in the following way. He assumes that chromatic excitations do not increase in proportion to their stimuli, but rather obey a law of diminishing returns (Fechner's law). This means that, for a given increase of stimulus, the weakest component of the retinal response will increase relatively more than the two stronger components. Accordingly, the colour becomes more whitish as the intensity is increased.

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[1] G. J. Burch, "On artificial temporary colour-blindness, with an examination of the colour sensations of 190 persons," *Phil. Trans. Roy. Soc.* B, 1899, cxcix, 50.

This hypothesis, however, would demand a continual decrease of saturation with increasing intensity, and indeed the saturation of a colour should be maximal at the threshold of vision. Actually, on the contrary, the saturation is very low at the threshold and only becomes maximal at a medium intensity.

Hering[2] explains the saturation changes in terms of the different sensitivities and the different quantities of the visual substances which his theory postulates. The 'white-black' substance is the most sensitive, and is decomposed at intensities which are too low to affect the two chromatic substances. Thus the spectrum appears grey in the neighbourhood of the absolute threshold. At very high intensities the available quantities of the three visual substances become exhausted. The chromatic substances, being present in smaller amount, are used up before the achromatic substance has reached its limit. Accordingly the spectrum approaches white as the intensity becomes very great. The maximum of saturation would be found at some intensity which is neither too great nor too small.

Thus the Hering theory is able to give a reasonable explanation for the general course of the saturation changes. Unfortunately, however, there is no way of formulating the predictions of this theory in quantitative terms. The Hering theory offers no basis upon which we can calculate the relative saturations of colours[3], and thus leads to no definite consequences which we can test by experiment.

It appears then that neither of the classical theories of colour vision offers us a fruitful working hypothesis for our present problem.

Although it is impossible to describe completely the changes of saturation with intensity in a quantitative fashion, there are two critical points in this series which are of particular importance and which can be experimentally determined. The first of these is the specific threshold, the lowest intensity at which colour is visible. It is of interest to know whether this point coincides with the general threshold, or whether there is a colourless interval, in the case of the rod-free area of the retina. A further question is whether this interval exists for all wave-lengths and, in general, what relative values it assumes for the various wave-lengths of the spectrum. This question is the starting point for my first series of experiments.

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[1] H. von Helmholtz, *Treatise on Physiological Optics* (Eng. trans., New York, 1924), II, 185 f.

[2] Cf. H. Ebbinghaus, *Grundzüge der Psychologie*, 3rd ed. (Leipzig, 1911), I, 275 f.

[3] Cf. Tschermak, *loc. cit.*

A second critical point is the intensity at which the saturation of a colour attains its maximum value. My next group of observations consists of measurements of this intensity for the different spectral colours.

The final series of experiments constitute a less direct approach to the problem. The comparison of brilliance is difficult when the colours in question have unlike hue and saturation; the difficulty is of the same type, though of less degree, as that involved in saturation judgments. We can, however, study the functioning of the brilliance mechanism by various indirect methods, such as the flicker method, which obviate the uncertainty of the direct method. This suggests the possibility of using some analogous procedure in the study of saturation.

If to a given coloured light we add a sufficient quantity of white light, the sensation of colour will be extinguished. For a fixed brilliance of coloured light one might reasonably expect the quantity of white light required for extinction to vary with the saturation of the colour, and in precisely the opposite sense. It might be presumed that the ratio between the intensity of the extinguishing white and the intensity of the coloured light would serve to describe the saturation of the colour. In that case we should have a perfectly general method for the comparison of saturations, regardless of hue and brilliance. In terms of this scale it would be possible to specify not only the relative saturations of the different spectral hues, but also the variations of saturation with the intensity of the stimulus.

With this idea in mind, I have made a series of determinations of the sort described for the various colours of the spectrum. Discussion of the meaning of these measures will be postponed until the results themselves have been reported.

II. DESCRIPTION OF APPARATUS.

The apparatus used in these experiments is designed to present a bipartite circular stimulus field to the observer's eye at E (Fig. 1). The left half of this field may be illuminated either by monochromatic light from the spectrometer S_1 or by white light from the lamp Z. The right half of the field may be illuminated by the spectrometer S_2. Separate illumination of the two halves is accomplished by means of the Lummer-Brodhun prism P, which is silvered upon the right half of its diagonal surface.

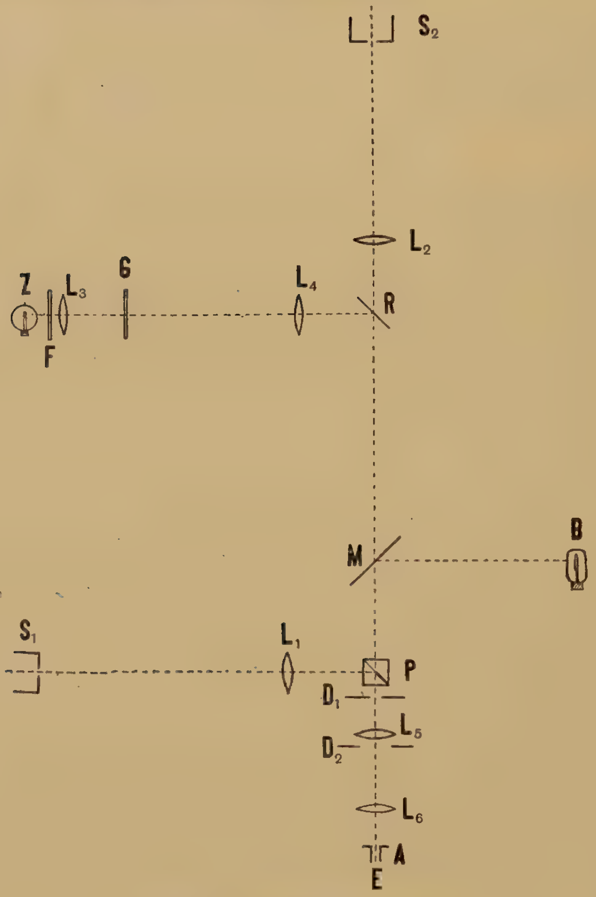

Fig. 1. Diagram of apparatus.

The collimator slits of the two spectrometers are illuminated by small incandescent lamps. By means of a slit at the ocular end, a narrow band of radiation is isolated from the spectrum of each instrument. These approximately monochromatic rays are made parallel by the lenses L_1 and L_2, respectively, before entering the prism. After emerging from the prism, these rays are converged by the lens L_5 in such a fashion that images of the two lamp filaments are formed, side by side, in the plane of the observer's pupil at E. The observer, looking through the artificial pupil A, sees an illuminated area when both spectrometer slits are illuminated. The colour of the left half of this area is due to S_1 and that of its right half to S_2. The size of the area is determined by the diaphragm D_1, which is just in front of the prism. The purpose of the lens L_6 is to assist accommodation.

A minute ink-spot on the centre of the lens L_5 serves as a fixation point. When very weak stimulus intensities are employed, a luminous point is substituted.

The lamp Z is provided with a blue filter F which renders its rays achromatic. The lens L_3 converges these rays to a focus, after which the lens L_4 makes them parallel. Finally, they are reflected towards the observer's eye by the glass plate R. A ground glass G is situated within the converging bundle from L_3. The intensity of light entering the eye can be varied by moving this glass towards or away from the focus. It is thus possible to change the intensity of the light without affecting its purity[1].

The lamp B, which is mounted upon a photometric bench, serves to measure intensities of stimulation. M is a magnesium-oxide sector disc with two open quadrants; its plane is inclined at 45° to the line of sight. Light from B reaches the eye by virtue of reflection at the surface of this disc. The disc may be rotated by means of an electric motor, and photometric measurements may thus be made by the flicker method. Where less accuracy is sufficient, the disc may be turned so that it furnishes steady illumination in the right side of the field, and the intensity received from S_1 may be measured by the equality-of-brightness method.

The intensities of the spectral stimuli in these experiments were controlled by varying the voltages across the lamps. Stimulus intensities were measured in terms of the actual illumination of the retina. The effective pupillary size was held constant by the use of the artificial aperture *A*, the breadth of which was less than the smallest diameter of the natural pupil. The measurements were expressed in terms of the photon, the unit introduced by Troland, and defined as that intensity of retinal stimulation which accompanies the use of a pupillary area of 1 square millimetre and an external stimulus brightness of one candle per square metre.

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[1] Before experimentation was begun, the illumination furnished by the lamp Z was matched with sunlight, introduced into the right half of the field by means of a heliostat and appropriate mirrors. The match was obtained by a suitable choice of filter and adjustment of lamp voltage. Thereafter the voltage was always held constant at this predetermined level whenever this lamp was used.

III. THE COLOURLESS INTERVAL.

Preliminary observation indicated that, in my own case, a colourless interval existed for all wave-lengths except those in the red end of the spectrum. This was true no matter whether the stimulus field had an area of 3°, $1\frac{1}{2}°$ or $\frac{1}{2}°$. The red hues showed a behaviour decidedly different from that of the others; these hues had a very distinct saturation even at the threshold of visibility. But this saturation was by no means equal to that produced by the same wave-lengths at medium intensities.

In order to determine whether other observers experienced a foveal colourless interval, the test devised by Burch was applied. The field (with a size of 1·5°) was illuminated with light of different wave-length on the two sides. For a first test the stimuli used were blue ($470m\mu$) and yellow ($575m\mu$), and for a second test they were green ($505m\mu$) and violet ($430m\mu$). To ensure purity of stimulus, monochromatic filters of the appropriate transmissions were placed in front of the spectrometer slits. These stimulus combinations, blue-yellow and violet-green, were chosen as providing maximum contrast and optimum conditions for the detection of a difference in hue. I began the experiment by adjusting the stimulus intensities until both sides of the field looked grey to me, and were of approximately equal brilliance. A luminous fixation point was used to assist the observer in maintaining central regard. After my subject had undergone 5 minutes' dark adaptation he was asked to observe these weak stimuli and to report what hue he saw in each side of the field. The actual wave-lengths employed were unknown to him.

In order to test for the existence of a colourless interval in the red, one side of the field only was illuminated, the wave-length being $700m\mu$. The observer fixated the luminous point, while I gradually increased the stimulus intensity until the semicircular field was just visible to him. At this moment he made a judgment as to colour.

The observers were Dr Karwoski and Messrs Harris, Oberlin and Volkmann of the Harvard Psychological Laboratory. In the case of every observer a low intensity could be found at which the 'blue-yellow' and 'violet-green' fields looked uniformly grey. In the case of the stimulus of

700$m\mu$, the observer invariably returned an emphatic answer that the field was distinctly red so long as it was visible at all. When he was asked to take special care to detect the existence of a possible grey phase as the stimulus intensity was gradually made subliminal, he always declared that no such phase existed; although with other colours such a phase was observed.

Thus for these four observers, as well as for myself, a foveal colourless interval is present in the yellow, the green, the blue and the violet, but is absent in the long-wave red.

In my own case I find that the specific threshold is lowered during the first few minutes of dark adaptation, but at the end of 5 minutes it reaches a constant value. A further 25 minutes of dark adaptation gives no measurable change in the threshold.

I have repeated Burch's experiment, already mentioned, on the effect of resting the eye in darkness for a prolonged period of time. As he recommends, I employed a bipartite field of two hues. For this purpose I chose green (505$m\mu$) and violet (430$m\mu$). As in the experiments with the colour pairs which I have described above, monochromatic filters were placed before the spectrometer slits, and the field size was 1·5°. To begin with, the intensities were lowered until the field looked uniformly grey. I then remained in darkness for 2 hours, and at the end of this period observed the same stimuli once more. I found that the bipartite field presented the same appearance as before; there was no suggestion of a hue on either side of the field.

Thus I am unable to confirm Burch's observation, although the spectral stimuli which I used would offer a more favourable condition for the appearance of hue than the less saturated coloured glasses which he employed.

I next determined, for my own eye, the relative magnitude of the colourless interval in the different parts of the spectrum. For a preliminary estimate, and as a check upon later quantitative measurements, the following simple procedure was followed. Pairs of stimuli of neighbouring wave-length (differing by about 10$m\mu$) were presented in juxtaposition, and for each member of the pair the intensity was adjusted to the lowest value that would yield a sensation of hue. These two threshold stimuli were then compared in respect of brilliance.

One can thus observe how the specific threshold varies with wave-length. Since the general threshold may be regarded as constant, the colourless interval will vary in the same sense as the specific threshold.

In this way I found that the colourless interval steadily increases from 625 to 580$m\mu$, but has approximately the same value for 580 and 570$m\mu$. From 570 to 560$m\mu$ there is a slight increase, but the interval scarcely changes throughout the spectral region between 560 and 490$m\mu$. In the blue and violet, from 490 to 430$m\mu$, there is a continual slight decrease, but never a complete abolition of the colourless interval.

For wave-lengths greater than 625$m\mu$, in the orange and red, the interval is very small. I have attempted to determine the critical wave-length at which the interval just begins to appear, as one passes from the spectral red towards the yellow. Starting with 700$m\mu$, where it is definitely absent, I decreased the wave-length of the stimulus by small steps. After each step I adjusted the intensity of the stimulus until the semi-circular field was barely visible, and made a judgment as to the presence or absence of hue. In order to ensure steady fixation, a luminous fixation point was used.

I found that hue was visible at the general threshold until I reached a point in the orange-red, at about 665$m\mu$. The mean variation in determining this point was about 2$m\mu$.

Finally, I have measured, in terms of the photon, the magnitudes of the general and specific thresholds for the different wave-lengths of the spectrum.

For a determination of the general or absolute threshold, the right half of the field was illuminated by the spectrometer S_1, and a luminous fixation point was provided. A magnetic shutter was placed so that, by closing a switch, one could cause it to intercept the rays from the spectrometer. When the intensity had been adjusted so that it was near the threshold value, the switch was alternately closed and opened, causing the faint spot of light to disappear and reappear. The threshold was taken as the lowest intensity at which a definite disappearance and reappearance could be observed. This procedure, due to Reeves, provides a valuable control upon absolute threshold determinations, which are peculiarly subject to errors of imagination. The general threshold was determined for thirteen wave-lengths ranging from 700 to 430$m\mu$.

Each determination of the threshold was preceded by five minutes' dark adaptation, experience having shown that after this time the values were practically constant. The 'ascending' and 'descending' thresholds were measured alternately. The lamp voltage corresponding to the threshold was read after each measurement. A graph representing *log* photons as a function of *log* volts had been plotted for each wave-length employed. This graph, throughout the range of voltages in question, is sensibly a straight line. Accordingly, the threshold voltages could be translated into photon values by extrapolation upon this graph.

The specific threshold was also determined for the same wave-lengths (with the exception of 700$m\mu$, at which value the specific and general thresholds coincide). In every case this determination was made by comparison with a grey standard presented simultaneously on the other half of the field and having an intensity of approximately 0·02 photon. This standard was furnished by the light of the lamp Z which was rendered achromatic in the manner already described. The value 0·02 photon was chosen as providing approximately optimal colour sensitivity, and minimal thresholds.

It must be borne in mind that the results obtained refer only to the particular conditions, including size of field and intensity of grey standard, which were used in these determinations. In particular, if observations were made in the absence of any standard light (as was the case in the experiments of Miss Monroe), a quite different set of values would have resulted. Thus, Miss Monroe remarks:

> The chromatic threshold for red is by far the easiest to determine. The transition from achromatic to chromatic is abrupt and sharply marked and there is little or no hesitation on the part of the observer as to whether or not there is any colour present. ...In the yellow just the opposite condition obtains. The colour comes in very slowly and gradually and the exact point of appearance of the chromatic component is difficult to determine. At low intensities light of wave-length 580$m\mu$ has much the appearance of ordinary artificial light, such as that emitted by a carbon lamp, which an unpractised observer is accustomed to consider as white. In such a case much greater intensities are needed for the light to be called coloured.

My purpose was to obtain conditions of maximum sensitivity and minimum variability, and a grey standard was therefore desirable.

The photon values of the threshold stimuli were obtained as before by the use of the voltage calibration curve.

The results, which are tabulated below and graphically represented in Fig. 2, refer to six observations of the general threshold, and eight of the specific threshold, for each wave-length. The 'ascending' and 'descending' thresholds were determined alternately.

The general threshold has an average value of about 0·005 photon. The specific threshold, which is zero in the red, rises rapidly in the orange, and shows no further pronounced variation in the remainder of the spectrum. There is however a slight decline in the violet. For the greater part of the spectrum the colourless interval lies between 0·01 and 0·02 photon.

Table I.

Wave-length $m\mu$	General threshold		Specific threshold		
	Mean	Mean variation	Mean	Mean variation	Colourless interval
	Photons				
700	0·0023	0·0009	0·0023	—	0
625	0·0043	0·0003	0·0052	0·0012	0·0009
600	0·0017	0·0003	0·0071	0·0025	0·0054
585	0·0032	0·0003	0·0178	0·0054	0·0146
560	0·0033	0·0005	0·0174	0·0035	0·0141
540	0·0053	0·0005	0·0238	0·0032	0·0185
525	0·0035	0·0016	0·0244	0·0048	0·0209
515	0·0070	0·0032	0·0229	0·0030	0·0159
500	0·0074	0·0013	0·0237	0·0029	0·0163
490	0·0072	0·0014	0·0228	0·0026	0·0156
470	0·0078	0·0010	0·0221	0·0023	0·0143
450	0·0049	0·0010	0·0179	0·0044	0·0130
430	0·0040	0·0010	0·0191	0·0009	0·0151

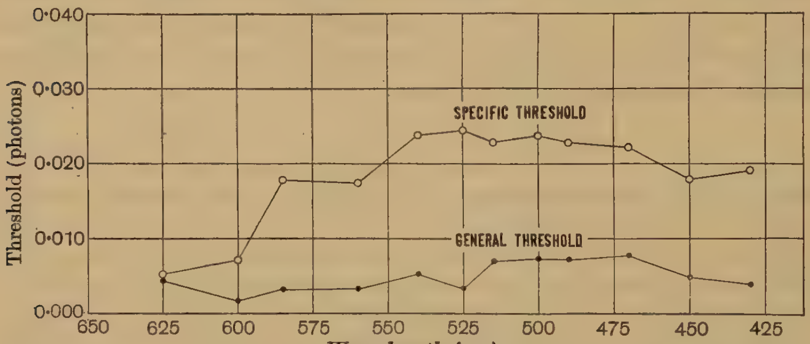

Fig. 2. The general and specific thresholds for the different wave-lengths of the spectrum. The axis of abcissae should be marked 'wave-length ($m\mu$).' The axis of ordinates should be marked 'Threshold (photons).'

Series of observations which were made with smaller field sizes (1·5° and 0·5°) gave higher values for both the specific and the general threshold, but did not indicate any marked change in the relative values of the colourless interval throughout the spectrum.

Let us now consider how the foveal colourless interval is to be theoretically interpreted. To begin with, let us determine whether the scotopic hypothesis proposed by Abney and Watson is adequate to the facts.

In favour of this hypothesis is the fact that the shortest wave-length in the spectrum for which the colourless interval is absent was found to have the value $665 m\mu$. For, according to the data of Hecht and Williams[1], this is almost precisely the longest wave-length of light which will elicit a response from the retinal rods. This makes it seem plausible that the presence of a colourless interval in the fovea may be due to the existence of a small number of rods in that region.

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[1] S. Hecht and R. E. Williams, "The visibility of radiant energy and the absorption spectrum of visual purple," *J. Gen. Physiol.* 1922, v, 1.

Other facts, however, furnish a preponderance of evidence against Abney and Watson's conception. In the first place, according to my observations, the foveal specific threshold is lowered by adaptation to darkness. One would expect adaptation to bring about an augmentation of achromatic scotopic function which would cause an increase, rather than a decrease, in the threshold for colour.

In the second place, I would particularly emphasize the fact that a reduction in the size of the stimulus field raises the specific threshold for the fovea. One can scarcely suppose that the proportion of rods becomes greater the more closely one approaches the centre of the fovea. There would be more reason to expect quite the contrary, if indeed the fovea contains any rods at all.

The problem is complicated, however, by the circumstance that the general threshold also varies with the size of the field. These relationships have been studied in some detail by Abney. In his *Researches in Colour Vision* (p. 155), Abney presents graphs showing the way in which the two thresholds depend upon the diameter of the stimulus, for foveal stimuli ranging between 1° 57′ and 1′ 30″.[1] The wave-lengths employed were 589 and 548$m\mu$. According to these data, both the logarithm of the specific threshold, and the logarithm of the general threshold, are linearly related to the logarithm of the field diameter; *i.e.* the two graphs, plotted in logarithmic terms, are straight lines. These two lines, however, have different slopes; that is to say, the two thresholds obey different laws in respect to the size of the field[2].

If for a given wave-length the two linear graphs, for the specific and general threshold respectively, are sufficiently prolonged, they will intersect. But one is not justified in drawing, as Abney does, the conclusion that "there is an angular aperture subtended on the retina at which any ray will be extinguished both for colour and light at the same time[3]." For, in the case of one wave-length (589$m\mu$), this intersection actually corresponds to a visual angle of 38″, and in the case of the other wave-length ($548 m\mu$), to a visual angle of 0·5″. The first angle is so small that a stimulus of that size would fall upon a single retinal cone. The second angle is so very much smaller as to make Abney's inference physiologically meaningless.

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[1] Due to an arithmetical error (which does not affect the accuracy of his graphs), Abney has described the two smallest fields as 9′ 9″ and 3′ 3″. It is clear from other data which he gives that the correct figures are 4° 30′ and 1′ 30″.

[2] The graphs yield the following equations: for the specific threshold, $i=a/d^{1\cdot 11}$, and for the general threshold, $i=b/d^{1\cdot 67}$, where i is the threshold intensity, d is the diameter of the field, and a and b are constants.

[3] P. 180.

Thus there is no reason to suppose that a sufficient diminution of the field diameter will abolish the colourless interval for the whole spectrum. And it is in any case certain that there is a colourless interval for ordinary point stimuli of less than spectral saturation. It is an experience familiar to astronomers that stars which appear coloured when viewed through a telescope of high magnifying power often seem colourless to the naked eye. The telescope of course simply increases the intensity of the starlight without magnifying the optical image.

Finally, it must always be remembered that the complete de-saturation of colours in the neighbourhood of the threshold is merely the limiting stage of the partial de-saturation which occurs at less extreme intensity. It is probable that the same principle which explains the colourless interval will also explain this partial de-saturation. But de-saturation occurs even in the case of red stimuli of wave-length greater than $665 m\mu$, which do not affect the retinal rods. Moreover, the spectral colours attain their greatest saturation only at an intensity which is thousands of times the general threshold. It is quite unlikely that the supposed foveal rods could contribute an appreciable proportion of the visual response to intensities of this order.

We cannot pause to review all the explanations of the colourless interval which have been proposed. Most of them, like that of Abney and Watson, ascribe the phenomenon to some peculiarity in the response of the individual receptors which appears at low intensity. There are reasons for believing that a hypothesis of this type is inadequate.

For example, Hering assumed that the colourless interval is due to the fact that the threshold of the 'white' process is lower than that of any of the chromatic processes. Since, however, the colourless interval is absent in the red, one would have to suppose that the threshold of the 'red' process is as low as that of the 'white' process. It is then difficult to see why there should be a colourless interval in the violet, since, on Hering's principles, the 'red' process is a component of the retinal response underlying the violet sensation.

To escape this difficulty, it may be postulated that colour is discerned only when the *proportion* of chromatic to achromatic excitation exceeds some critical value. But one who makes such a postulate is not explaining the phenomena of colour sensation solely in terms of individual receptor processes; he is introducing a factor of discrimination. The specific threshold would be ultimately dependent on the ability to differentiate between an achromatic excitation which is pure and one which is adulterated with a slight trace of chromatic excitation.

An assumption of this sort was indeed made, in an incidental fashion, by Hering in his early writings. "Sinkt der Bruchtheil, welcher vom Gesammtgewichte einer Empfindung auf eine ihrer Componenten kommt, unter einen gewissen Werth, so sind wir nicht mehr im Stande, diese Componente als solche herauszufühlen....Im Schwarz und Weiss sind eben die gleichzeitig mit empfundenen Farben 'unter der Schwelle,' weil ihr relatives Gewicht zu klein ist[1]."

Helmholtz, in attempting to account for the colourless interval, once expressed himself in a somewhat similar fashion. "Es erklärt sich... dadurch, dass zur Unterscheidung der Helligkeit nur die ganze vorhandene Lichtmenge von absoluter Dunkelheit unterschieden werden muss. Zur Unterscheidung einer kleinen Menge Weiss von einem farbigen Licht müssen dagegen Verhältnisse von Lichtmengen zweier Grundfarben von einander unterschieden werden[2]."

The fact that the specific threshold is determined by the proportion, rather than the absolute amount of chromatic response, is clearly shown by the relatively large foveal colourless interval which is observed in the case of unsaturated colours. Thus Donders and Aubert found a very considerable difference between the general and specific thresholds in the case of pigment colours for foveal vision. A simple experiment will suffice to show the dependence of the colourless interval upon saturation. If a coloured paper is mixed upon a rotating disc with varying amounts of an equally brilliant grey, the reduction of illumination required to extinguish colour will be found to decrease as the grey sector is enlarged.

A smaller colourless interval is naturally to be expected in the case of the colours of the spectrum on account of their greater saturation. But there is no reason to think that any different principle is involved, or that any special hypotheses must be made in order to explain the behaviour of these colours. Moreover, the distribution of specific thresholds in the spectrum (Fig. 2) can be simply accounted for. The more saturated a colour, the lower will be its specific threshold, or, in other words, the same factor that causes a spectral hue to appear saturated at higher intensities makes it retain its saturation as the intensity is made very low.

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[1] E. Hering, *Zur Lehre vom Lichtsinne* (Vienna, 1878), p. 85. I am indebted to Dr E. G. Boring for calling my attention to this passage.

[2] "Kürzeste Linien im Farbensystem," *Zschr. f. Psychol.* 1892, III, 108. See p. 123.

The absence of the colourless interval in the red can now be given a simple interpretation, since the red is distinctly the most saturated colour in the spectrum. In the case of this colour the proportion of chromatic to achromatic component never sinks below the threshold of discrimination.

We have seen that this idea, that colour perception is based upon discrimination, was expressed by Hering and Helmholtz. It has also been stated, and with more explicit recognition of its significance, by Johannes von Kries. In discussing the problem of colour thresholds, this author says:

> One can explain these facts (in the usual fashion of the Hering theory) by assuming that the total value of the stimulus for the colour sensing portion of the visual organ falls below the threshold. But if we conceive the substrate of colour sensation in this manner, as an independent component, then it becomes incomprehensible, and in contradiction with all our other physiological knowledge, that when we vary the stimulating light...*qualitatively*, the threshold value is fixed not by an absolute quantity of coloured light, but by a certain ratio of coloured to white. Since we can scarcely conceive of a peripheral sensory apparatus for which this *ratio* shall determine the threshold, we are forced to adopt the notion that the colour threshold is of a different sort from most of the other sensory thresholds with which we are familiar...[1].

On Hering's principles, we must suppose the colourless interval to be determined by two factors, first, the increased proportion of achromatic process at low intensities, and second, the dependence of colour sensation upon the ratio between chromatic and achromatic process. But there is reason to believe that this first factor need not be assumed, and that a simpler and more likely hypothesis can be found which is based upon the second factor alone.

Namely, it may be that the critical ratio between chromatic and achromatic process[2] which must be surpassed in order that colour shall be discriminated is itself a function of intensity. In other words, a reduction of the intensity of stimulus lowers the discriminative power of the visual mechanism.

It is a well-known fact that low intensity introduces a weakening of other visual discriminatory functions. The ability to discriminate intensities, as well as spatial and temporal differences of stimulation (cf.

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[1] "Die Gesichtsempfindungen," *Nagels Handb. d. Physiol.* (Brunswick, 1905), III, 272. It ought to be said that von Kries has the thresholds of peripheral vision in mind in this passage; but his logic could be applied with equal justice to the foveal colourless interval.

[2] Our reasoning here is not based upon the Hering conception, or any other special conception, of the nature of achromatic process.

the data on visual acuity and critical flicker frequency) is lowered. It is possible that the de-saturation and ultimate extinction of colours may be associated with these other discriminatory defects by way of a common physiological factor.

Moreover, there are other ways, besides reducing the intensity, in which a colour may be extinguished. With a fixed value of intensity, one can force a colour below the specific threshold by diminishing the area of stimulation—just as, with a fixed area, one can produce the same effect by lowering the intensity. Within limits, at any rate, area and intensity act reciprocally.

This suggests that the two factors, area and intensity, may have a basically similar action, in that they both affect the discriminatory mechanism. There is still a third factor which may be compared with these two, namely time. If the time of exposure is too brief, one sees grey rather than a colour; there is a 'temporal colourless interval.' This interval may be reduced by increasing the area of the stimulus[1], or by increasing its intensity[2].

While it is possible to interpret all these relationships in terms of special assumptions regarding the behaviour of the independent achromatic and chromatic processes of the Hering theory, it seems simpler and less arbitrary to regard them in the light of the unitary principle of 'discrimination' which has been proposed here.

It is suggestive that there is a phenomenon analogous to the colourless interval in the domain of auditory sensation. If the loudness of a pure tone is reduced sufficiently, the pitch of the sound becomes indeterminate, and the sensation degenerates into a noise. Also sounds of very low intensity require a larger minimum number of vibrations to produce an experience of pitch.

With these facts in mind, we realize that the disappearance of colour at low intensity is not an isolated phenomenon, but rather a special case of the degeneration of quality under unfavourable conditions. It may be doubted, then, whether we need to postulate special photochemical factors, or indeed any factors which are unique to the visual mechanism, in order to account for the colourless interval.

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[1] A. Brückner and R. Kirsch, "Untersuchungen über die Farbenzeitschwelle," *Zschr. f. Sinnesphysiol.* 1912, XLVI, 364.

[2] H. Piéron, "Lois de l'établissement du chroma des impressions lumineuses," *C.R. Acad. Sci.* 1929, CLXXXIX, 194.

IV. THE INTENSITY REQUIRED FOR MAXIMUM SATURATION.

The only quantitative data relating to the maximal saturations of the spectral colours are those of I. A. Haupt[1]. This experimenter found the rather striking result that the spectral yellow ($580 m\mu$) reaches its greatest saturation at a very high intensity, as compared with the other colours. For wave-lengths on either side of the spectral yellow the intensity corresponding to the maximum sank to a relatively low level, which showed no pronounced variation with wave-length. Of the seven wave-lengths employed, ranging from 655 to $463 m\mu$, the wave-length $463 m\mu$ (blue) was found to reach its maximum saturation at the lowest intensity[2]. Haupt has also measured the number of just noticeable saturation differences which accompany the increase of intensity of a spectral colour from the specific threshold up to the point of maximum saturation. The order of the colours in regard to the number of saturation steps proved to be just the reverse of that obtaining in the intensity measurements, *i.e.* blue had the greatest number of steps and yellow the least.

I have thought it desirable to make a series of determinations of the point of maximum saturation, employing a large number of wave-lengths including the whole spectral range. The method employed was a simple one. The intensity of spectral light was varied by means of the rheostat in series with the spectrometer lamp; beginning with an intensity below the critical point, the intensity was raised fairly rapidly until a point was reached at which the saturation of the colour no longer increased but began to decline. Twenty wave-lengths, varying from 700 to $440 m\mu$, were studied, and fifteen observations of the maximum point were made with each wave-length. The field size was 3°.

This kind of observation is exceedingly difficult, and no great accuracy can be claimed for the results. They are probably adequate, however, to give a general characterization of the manner in which the maximum point varies for the different wave-lengths. The data are listed in Table II, and are graphically described in Fig. 3.

The results are in broad agreement with those of Haupt, in that the curve has a maximum in the middle of the spectrum, and drops to low values at the spectral end-regions. From these data we can draw a rough generalization: the more saturated a hue, the lower the intensity at which its saturation becomes a maximum. This rule is not rigorously valid, because the violet reaches its maximum at a lower intensity than the red.

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[1] "The selectiveness of the eye's response to wave-length and its change with change of intensity," *J. Exp. Psychol.* 1922, v, 347.

[2] Determinations were made both with a grey background illumination and without it, the two conditions yielding quite different results. I am quoting only the former series.

Table II.

Wave-length ($m\mu$)	Intensity required for maximum saturation (photons)	Wave-length ($m\mu$)	Intensity required for maximum saturation (photons)
700	42	550	181
660	44	540	193
635	36	525	155
620	57	515	85
600	49	505	62
592	64	490	19
585	134	480	24
575	175	470	15
565	219	460	12
555	182	440	9

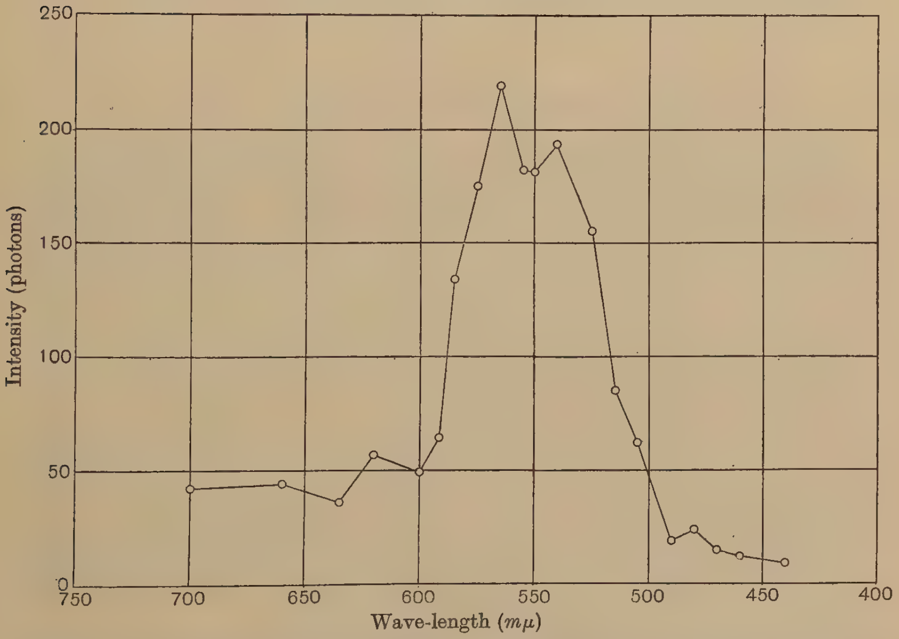

Fig. 3. The intensity required to produce maximum saturation, as a function of wave-length. The axis of abscissae should be marked 'Wave-length ($m\mu$).' The axis of ordinates should be marked 'Intensity (photons).'

The explanation of this relationship is difficult. Following the hypothesis already suggested, we may interpret the diminution of saturation at lower intensities as a direct effect of weak light upon qualitative discrimination. Some other factor is needed to account for the decrease of saturation at high levels of intensity, and thus for the existence of a maximum. (The hypotheses proposed by Hering and Helmholtz may be recalled here.)

It is of interest to note that there is a similarity between the behaviour of colours with respect to intensity increase and with respect to fatigue. H. Sheppard has reported[1] some experiments in which the eye was fatigued with colour for a time sufficient to produce complete desaturation. The time required to destroy the sensation of colour varied in the case of different colours of equal brightness, being least in the violet and blue, somewhat greater in the red and blue-green, and greatest in the yellow. The same ranking appears here as is exhibited in Fig. 3. The colours which lose their saturation first as the intensity is raised are also most quickly extinguished by chromatic fatigue; paradoxically, the highly saturated colours are most susceptible to both influences.

V. THE CHROMATIC THRESHOLD WITH RESPECT TO WHITE.

The next experiments consisted of measurements of the quantity of white light which when added to a spectral colour will suffice to extinguish the colour sensation. In such a measurement one can proceed in either of two ways. The quantity of colour may be held constant, and a variable amount of white added to it; or to a fixed amount of white one may add varying amounts of colour. The latter procedure is more convenient in practice, and was adopted here. Stated in different terms, applying more directly to the actual conditions of observation, the experiments consisted of determinations, for a fixed quantity of white light, of the least addition of coloured light which could be perceived. We may describe this just noticeable addition as the 'chromatic threshold with respect to white.' The ratio between this threshold and the intensity of the white light may be called the 'fractional chromatic threshold.' It was thought that this fractional threshold might be found to vary inversely with the saturation of the colour.

At the time of making these experiments, I was unaware of the recent work of Priest and Brickwedde[2] along similar lines. These investigators have made very precise measurements of the spectral chromatic thresholds, under somewhat different conditions from those involved in my experiments. As yet their complete data have not been published, and the results have appeared only in the form of a brief abstract. There appears, however, to be a degree of agreement between our results which is satisfactory, considering the unlike experimental arrangements, and the cruder technique employed by myself.

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[1] "Foveal adaptation to colour," *Amer. J. Psychol.* 1920, XXI, 34.

[2] I. G. Priest and F. G. Brickwedde, "The minimum perceptible colorimetric purity as a function of dominant wave-length with sunlight as neutral standard," *J. Opt. Soc. Amer.* 1926, XIII, 306.

Apparently Priest and Brickwedde have determined the chromatic thresholds for only a single intensity of the white stimulus. I have followed the variations of the chromatic threshold with intensity as well as with wave-length.

According to G. Révész[1], the chromatic threshold for any given wave-length is a linear function of the intensity of the initial white stimulus. Unfortunately, Révész's measurements are expressed in terms of arbitrary units, which makes it impossible to draw any comparisons between the results for different colours. Hence it seemed desirable to make further measurements of this sort, employing the uniform scale which is furnished by units of retinal illumination.

The following procedure was adopted. The lamp *Z* was used as a source of white light, as in the experiment on the colourless interval. The intensity of stimulation received from it was controlled by moving the ground glass *G*. A centimetre-scale was attached to the framework on which the ground glass moved, so that its distance from the lamp could be read. A calibration of intensity *versus* distance was then carried out. Any desired intensity could then be obtained by setting the ground glass at the distance indicated by the calibration curve.

For the spectral colour under investigation, a calibration curve, showing the relation between intensity and the voltage of the spectrometer lamp, was obtained by the flicker method. Voltmeter readings could then be translated into intensities by means of this curve.

The intensity of white light was first adjusted to some predetermined value. Monochromatic light was then gradually added, its intensity being varied by means of the rheostat in series with the spectrometer lamp, and the point was determined at which a tinge of colour was just visible. After a series of observations of this sort, another series was made in which a supraliminal admixture of spectral light was reduced until the field appeared pure white. Ordinarily, twelve observations were made for each wave-length at each intensity—six of the 'ascending' and six of the 'descending' threshold—although in the case of some of the colours having smaller thresholds twenty-four determinations were secured. The average of all observations for a given wave-length was taken as representative of the threshold. The stimulus field subtended a visual angle of 1·5°.

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[1] "Über die Abhängigkeit der Farbenschwelle von der achromatischen Erregung," *Zschr. f. Sinnesphysiol.* 1907, XLI, 1.

The data are reproduced in Table III. In the rows marked T., I have given the value of the chromatic threshold in photons. Directly under each T. value, in the row marked R., will be found the corresponding ratio between the white stimulus and the threshold chromatic stimulus (*i.e.* the reciprocal of the fractional chromatic threshold). Thus, for the wave-length 660$m\mu$, the chromatic threshold is 4 photons when the white stimulus has the value 200 photons, and the reciprocal threshold is 50. The just perceptible proportion of colour is 1/50.

Table III.

Wave-length ($m\mu$)		Intensity of white (photons)											
		40	70	100	200	300	400	500	600	700	1000	1500	2000
660	T.	—	—	—	4·0	7·0	8·4	11·0	13·0	13·7	—	—	—
	R.	—	—	—	50·0	42·9	47·6	45·5	46·2	51·1	—	—	—
630	T.	—	—	—	7·3	10·8	13·3	15·4	18·0	25·5	—	—	—
	R.	—	—	—	27·4	27·8	30·1	32·5	33·3	27·5	—	—	—
605	T.	—	—	4·0	18·0	24·6	35·8	49·0	65·0	81·0	—	—	—
	R.	—	—	25·0	11·1	12·2	11·1	10·2	9·2	8·6	—	—	—
575	T.	8·0	9·9	12·7	29·0	50·0	92·0	120·0	170·0	230·0	—	—	—
	R.	5·0	7·1	7·9	6·9	6·0	4·3	4·2	3·5	3·0	—	—	—
540	T.	—	—	4·8	16·2	31·2	42·0	60·0	72·0	91·0	—	—	—
	R.	—	—	20·8	12·3	9·6	9·5	8·3	8·2	7·7	—	—	—
505	T.	—	—	—	15·5	25·0	37·3	52·0	60·0	—	—	—	—
	R.	—	—	—	12·9	12·0	10·7	9·6	10·0	—	—	—	—
485	T.	—	—	—	10·4	15·8	20·0	27·8	35·0	55·0	—	—	—
	R.	—	—	—	19·2	19·0	20·0	18·0	17·1	12·7	—	—	—
470	T.	—	—	—	6·4	7·6	10·0	12·6	15·0	18·7	—	—	—
	R.	—	—	—	31·2	39·5	40·0	39·7	40·0	37·4	—	—	—
455	T.	—	—	—	2·3	—	3·3	—	4·6	—	7·1	12·3	16·
	R.	—	—	—	87·0	—	121·0	—	130·0	—	141·0	122·0	120·
440	T.	—	—	—	1·5	1·9	2·4	2·8	3·4	4·1	4·6	10·3	16·
	R.	—	—	—	133·0	158·0	167·0	178·0	176·0	171·0	167·0	146·0	125·

In Fig. 4, a typical set of data is plotted, corresponding to a white of 500 photons. The reciprocal of the fractional chromatic threshold is represented as a function of wave-length.

This curve yields an interesting and unforeseen relationship. Namely, its form is strikingly similar to that of a curve obtained by R. H. Sinden[1] in his study of complementary colours.

Sinden has determined, for various spectral complementary pairs, the relative intensities of the components required to produce a colourless mixture. He assumes that these intensities are in the inverse ratio to the respective saturations of the two components. By a process of extrapolation he establishes the relative 'saturations' of all the spectral colours in terms of a common unit.

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[1] "Studies based on the spectral complementaries," *J. Opt. Soc. Amer.* 1923, VII, 1123.

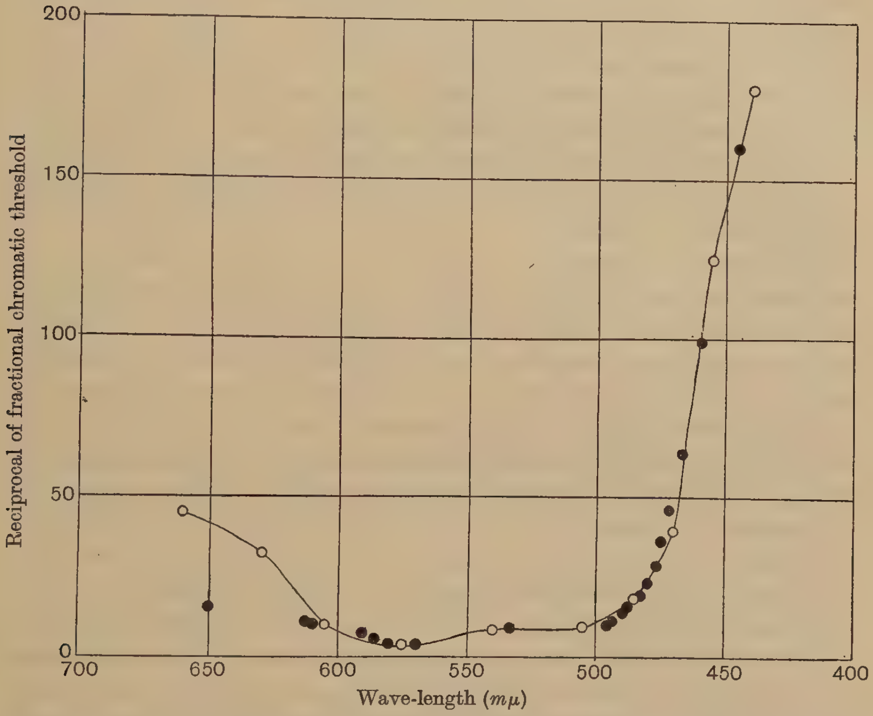

Fig. 4. The reciprocal threshold as a function of wave-length, and its relation to complementation valence. White circles = reciprocal of fractional chromatic threshold. Black circles = complementation valence, according to Sinden. The axis of abscissae should be marked 'Wave-length ($m\mu$).' The axis of ordinates should be marked 'Reciprocal of fractional chromatic threshold.'

In accordance with the remarks made at the beginning of this paper, the present writer would prefer to use the term 'saturation' only in its psychological sense. 'Complementation valence' would be preferable as a description of the quantity determined by Sinden.

To bring out the similarity between the data on chromatic threshold and those on complementation valence, I have multiplied Sinden's measures by an appropriate constant (equal to 4), and have represented them on the same diagram with my own observations (Fig. 4). It will be seen that the agreement is rather good, the only marked discrepancy occurring at the red end of the spectrum[1].

This concordance of measures implies the following principle: The chromatic threshold of a hue is inversely proportional to its complementation valence. In particular, the chromatic thresholds of two complementary hues are in the same ratio as the appropriate intensities required for complementation.

Thus, for example, a violet hue needs but a relatively small intensity in order to cancel the chromatic effect of a greenish yellow and produce a neutral mixture. The same physiological factor governs the size of the chromatic threshold, and is responsible for the fact that only a very small amount of violet need be added to a white in order to produce a perceptible tinge of colour.

The writer has recently learned that Dr Selig Hecht has anticipated him in arriving at this relationship, and has embodied this finding in a paper read before the Optical Society of America, and as yet unpublished. Dr Hecht has shown how the connection between chromatic threshold and complementation valence may be quantitatively predicted on the basis of a new conception of the trichromatic theory which he has worked out, and has verified this prediction by a comparison of the data of Sinden with those of Priest and Brickwedde. This work promises to be of the greatest interest for students of colour vision.

I shall venture to point out a certain difficulty which appears to me to be involved in the interpretation of the chromatic threshold according to Young-Helmholtz principles.

The most reasonable assumption one can make regarding the chromatic threshold is that the quantity of chromatic excitation required to make a colour just perceptible is always the same, no matter what the wave-length[2].

For purposes of illustration, let us apply this assumption to a very simple case. Suppose that a certain wave-length, λ_1, produces an effective[3] chromatic excitation represented by 3 units of red and 1 unit of green. Its complementary wave-length, λ_2, when adjusted to the proper intensity for complementation, will produce 3 units of blue and 2 of green. For the complementary mixture must contain equal amounts of red, green and blue. Now, in this mixture, the total chromatic excitation due to λ_1 is 4 units, whereas the total chromatic excitation due to λ_2 is 5 units.

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[1] The two sets of observations were made under closely comparable conditions. The field size was the same (1·5°), and the threshold chromatic stimuli for the 500-photon white had approximately the same intensities as the corresponding complementary stimuli in Sinden's experiment.

[2] The 'constant quantity hypothesis' is fundamental in Hecht's theory of vision. He has presented cogent evidence in favour of the view that brilliance thresholds (absolute or differential) always involve a constant increment of photochemical change, and it is plausible to assume that a similar principle applies to the chromatic threshold.

[3] *I.e.* neglecting the equal moieties of red, green and blue which cancel one another and form white.

But the respective chromatic thresholds, by hypothesis, correspond to *equal* amounts of chromatic excitation and not to a ratio of 4 : 5. Thus on the Young-Helmholtz theory we should expect the threshold quantities of two complementary lights to bear a different ratio from that in which the two lights must be mixed in order to produce white. Experiment, however, shows that the ratio is the same.

It is possible to reconcile the theory with the facts by making a very special assumption. Namely, it can be assumed that the chromatic threshold is determined not by the total quantity of chromatic excitation, but only by that of the *dominant* excitation. Then, in the case of λ_1, the threshold would be established by the presence of a certain definite quantity of 'red' excitation, and in the case of λ_2, by an *equal* quantity of 'blue' excitation. The subordinate 'green' components would have no influence upon the just noticeable difference. On this hypothesis the threshold excitations of two complementary colours would correspond exactly to the respective excitations required for a neutral mixture.

The hypothesis, however, is arbitrary, and does not seem plausible on its own merits. The facts are interpreted more directly in terms of Hering's theory, or any theory which assumes that equal amounts of chromatic excitation have equal cancelling power. Thus, according to Hering, the wave-length λ_1 will cause a certain concentration change in the red-green substance, and a certain concentration change in the yellow-blue substance. The wave-length λ_2, when adjusted to the proper intensity so that it will neutralize λ_1, must produce concentration changes in the two substances which are precisely equal (and opposite) to those caused by λ_1. On the other hand, according to our 'constant quantity' hypothesis, the amount of change corresponding to the chromatic threshold is the same for all wave-lengths. Thus it follows that the chromatic thresholds of two complementary hues are proportional to their complementation intensities.

Let us next consider the relation of the data to the problem of colour saturation. The curve of Fig. 4 certainly bears some resemblance to the distribution of saturations in the spectrum in that it has its minimum in the yellow, and rises towards the spectral end-regions. In my judgment, however, the correspondence is not complete, the values in the violet being entirely too high as compared with the red. The curve appears to describe some physiological variable which is closely associated with saturation, although not identical with it.

If the chromatic threshold for a given wave-length is plotted as a function of the intensity of the white stimulus, one obtains a curve of the type shown in Figs. 5 and 6. The first of these figures contains the graphs for the longer wave-lengths of the spectrum, and the second contains those for the shorter wave-lengths. The graph for 575$m\mu$ appears in both figures, and serves as a standard of reference.

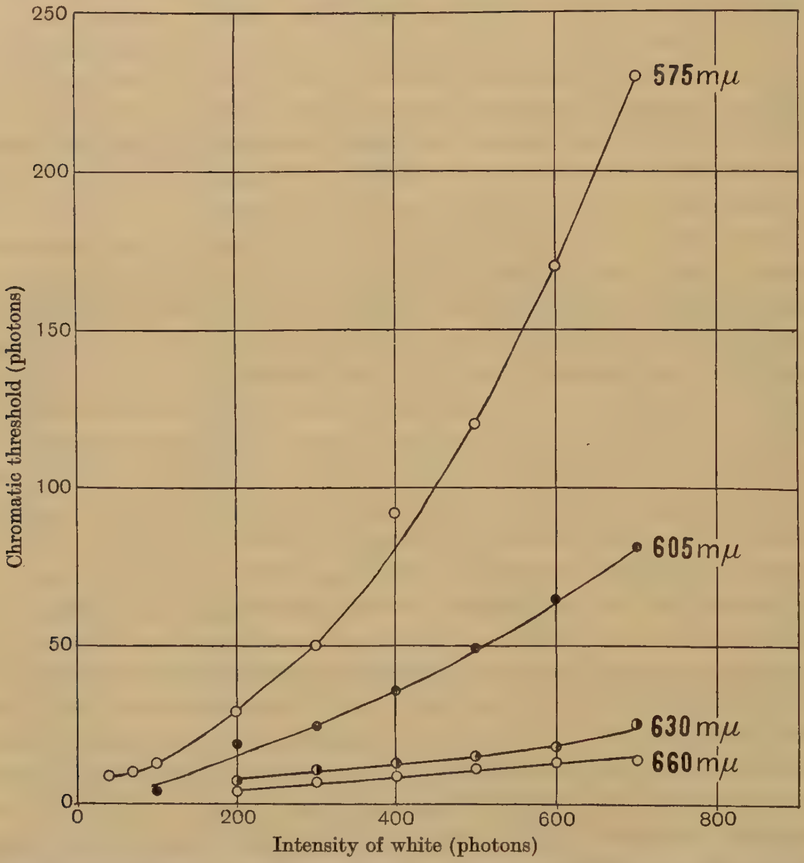

Fig. 5. The chromatic threshold as a function of the intensity of the white stimulus, for the wave-lengths 660–575$m\mu$. The axis of abscissae should be marked 'Intensity of white (photons).' The axis of ordinates should be marked 'Chromatic threshold (photons).'

According to these results, the relationship between chromatic threshold and intensity of white is not strictly linear, as Révész found it. There is a definite upward curvature of the lines, which is most pronounced in the case of the middle wave-lengths. Both the slopes of the lines and their degrees of curvature vary along the spectrum in the same sense that the chromatic thresholds themselves vary, being greatest for the middle wave-lengths, and least for the extreme wave-lengths.

These data may be represented in a different way. As mentioned previously, it was the writer's expectation that the reciprocal of the fractional chromatic threshold might prove to be valid as a perfectly general specification of colour saturation. In our Table III, this reciprocal is given in the rows marked R. Now, let us consider how this reciprocal varies as a function of the intensity of chromatic stimulus, *i.e.* how R. varies with T.

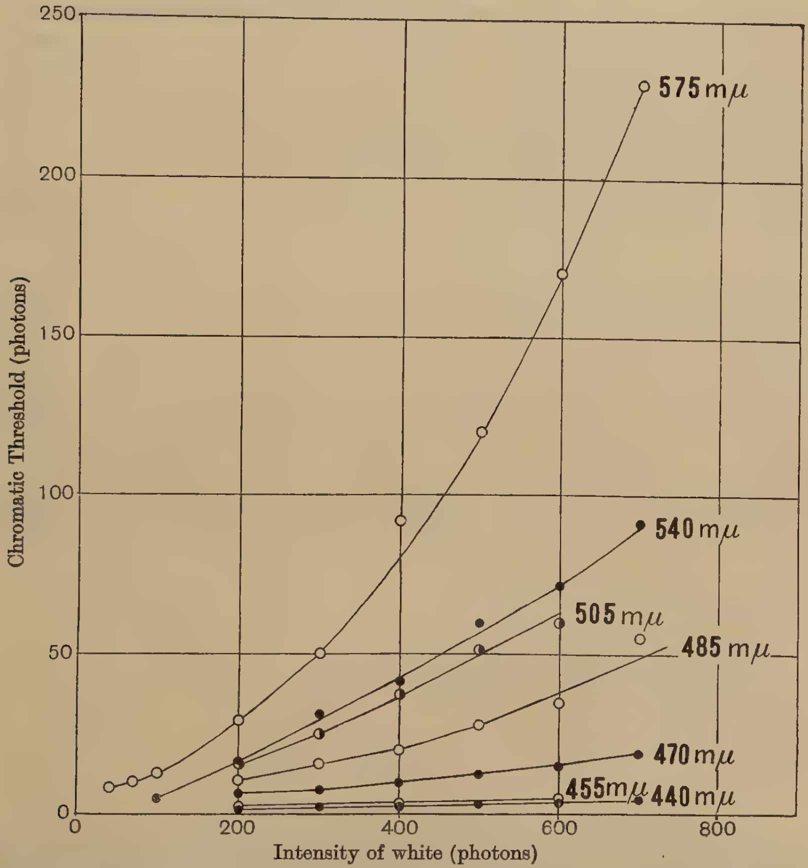

Fig. 6. The chromatic threshold as a function of the intensity of the white stimulus, for the wave-lengths 575–440$m\mu$. The axis of abscissae should be marked 'Intensity of white (photons).' The axis of ordinates should be marked 'Chromatic threshold (photons).'

It will be seen that, while the values are subject to irregular fluctuations, in most cases there is a definite indication of a downward trend at the higher intensities. Also, there is an indication (though less definite) that R. increases with intensity at the outset and rises to a maximum before it decreases. This maximum, however, does not occur at the same intensity level at which the psychological saturation is a maximum. Thus, *e.g.* for the wave-length $575 m\mu$, the reciprocal threshold continually increases between the chromatic stimulus intensities 29 and 230 photons. According to my observations of the actual appearance of this hue (cf. Table II), however, its saturation is constantly rising until an intensity of 175 photons is reached, and only then begins to decline.

We have already noted that the reciprocal threshold does not agree with psychological saturation in respect to its distribution in the spectrum. It now appears that there is a further disagreement between the two, namely in regard to their changes with intensity. Thus we are forced to conclude that the chromatic power or chromatic valence of a colour, as measured by its threshold with respect to white, or by its proportional intensity in a complementary mixture, is not identical with its saturation.

One might propose to measure this chromatic power in a different manner. To a spectral colour of fixed intensity there might be added a quantity of white light sufficient to cause a just noticeable *de-saturation*. This quantity, it would seem, ought to be greater for the more saturated colours. Nevertheless, experiment[1] seems to indicate that the threshold quantity depends simply on the brilliance of the colour and is independent of its saturation; presumably the threshold would be precisely the same for all colours of equal brilliance.

These results, taken in conjunction with my own, point to the necessity of a 'physiological,' rather than a 'psychological' interpretation of the threshold.

VI. SUMMARY.

1. Experiments are described which indicate the presence of a colourless interval in the fovea for spectral stimuli of wave-length less than $665 m\mu$. For greater wave-lengths, in the red end of the spectrum, the interval is absent.

2. The specific threshold is decreased by dark adaptation, but (with the exception just mentioned) does not coincide with the general threshold even after 2 hours in darkness.

3. The value of the general and specific thresholds (in photons) was measured for the different wave-lengths of the spectrum.

4. It is pointed out that the complete de-saturation of colours near

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[1] Cf. Tschermak, *loc. cit.*

the threshold of visibility is only the limiting stage of the partial desaturation which takes place at moderately low intensities. It is suggested that this phenomenon may be most satisfactorily interpreted as the result of the inadequacy of a discriminatory mechanism. The influences which stimulus, area and exposure time, as well as intensity, exert upon the perceptibility of colours, are considered in the light of this unitary principle.

5. Measurements were made, for the different wave-lengths of the spectrum, of the intensity which is required to produce maximum saturation. This intensity is small in the red, rises to a high value in the middle of the spectrum, and sinks to its lowest value in the violet. Roughly speaking, the more saturated a colour, the lower the intensity at which its saturation becomes a maximum.

6. To a given intensity of white light, there was added an amount of spectral light sufficient to produce a just noticeable colour. This threshold amount was found to be greatest in the yellow, and least in the violet, where it is exceedingly small. If we plot a curve representing the reciprocal of the threshold as a function of wave-length, this curve proves to be closely similar to that obtained by R. H. Sinden for the complementation valences of the spectral colours. It follows that the chromatic threshold of a hue is inversely proportional to its complementation valence; the chromatic thresholds of two complementary hues are in the same ratio as the respective intensities of those hues which are required for complementation.

7. This relationship may be readily interpreted in terms of the Hering theory, or any theory which assumes that two chromatic excitations which complement each other must be equal in total amount. Its interpretation in terms of the Young-Helmholtz theory, according to which balanced chromatic excitations are in general unequal, meets with considerable difficulty.

8. If for a given wave-length the chromatic threshold is plotted as a function of the intensity of the white stimulus, an upwardly concave curve is obtained. This result is contrary to that of G. Révész, who found a linear relationship. The slopes and accelerations of these curves are greatest for the middle wave-lengths, and least for the extreme wave-lengths, varying along the spectrum in the same sense that the chromatic thresholds themselves vary.

9. The fractional chromatic threshold is not perfectly correlated with subjective colour saturation, but appears to be closely related to it.

(*Manuscript received* 3 *May*, 1930.)

SOME EXPERIMENTS WITH WOOL TEXTILE TRADE ADVERTISEMENTS[1].

BY HENRY BINNS.

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II. *The methods of the investigation* (pp. 316–317).
  (*a*) *The test advertisements* (p. 316).
  (*b*) *The instructions to judges* (p. 316).
  (*c*) *The subjects* (pp. 316–317).
  (*d*) *The conditions* (p. 317).
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III. *The results and conclusions* (pp. 317–324).
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  (*d*) *Combination of factors* (p. 325).

I. THE JUDGMENT OF SPECIALITY MATERIALS.

THE advertising of articles which can be recognized by name and usually by a fixed price has already been investigated thoroughly by advertising experts. But the selling of articles (*e.g.* wool-tops—combed wool) by specialists having a detailed knowledge of one particular class of material to other specialists whose machinery is adapted to that class, comes under a different category. Both persons involved in the sale thoroughly understand the general possibilities of the material, but the buyer is not aware of the blends of wool which are present in the top. Moreover quality judgment is definitely associated with price—which sometimes fluctuates rapidly—and also with time of delivery, length of credit, etc.

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[1] This paper was read to the psychological section of the British Association for the Advancement of Science, Bristol, 1930.

Within limits, competition revolves around the price of a number of qualities offered by different persons or firms. As wool is sold by auction, and is treated in scouring and combing by one of several methods, there can be no guarantee that a quality bought at one time can be repeated at a later date, though a general standard of quality is recognized by the trade. In advertising such goods—which are typical of most classes found during the processing of wool textiles—the cost of advertising must be included in a competitive market price.

There is no monopoly except in so far as the particular blends in one top may meet the peculiar needs of a buyer and cannot be obtained elsewhere.

The problem is whether it is possible to create such an impression on the mind of a buyer, in an advertisement, as will lead him to buy goods after comparison, at a competitive price, which may be reduced by increased sales or increased by advertising costs.

As the object of advertising is to open up new business the probability is that no direct orders will result, for the quality of the new material must first be judged by sight and touch. The advertisement, if properly laid out, should be an introduction for a direct representative or an agent; it might also result in enquiries for samples by post.

In general advertising such as that of soaps, toffees, motor cars, etc., the technical mind endeavours to influence the natural and untrained judgment. This means that wise, sound, biased and non-suggestible judgments are seeking to influence relatively partial, weak, erratic and suggestible judgments. Thus the tendency is to make use of illustrations which stimulate the instincts and sentiments without drawing too much on reason.

It is unusual for the specialist-to-specialist textile advertisement to attempt more than to influence the mind through vision. Stimulation of the sense of touch, and the appeal to memory are lacking. Reason does not enter largely, and the appeal tends to drift into a mere statement of the barest facts. Displays and illustrations are designed to attract momentary attention, rather than to provide a comprehensive and pleasing effect which may leave a permanent impression.

Since little experimental work has yet been done on the technical side of textile advertising, it seemed probable that some fundamental factors of practical utility might result from an investigation.

II. THE METHODS OF THE INVESTIGATION.

(*a*) *The text advertisements.*

It was possible to obtain proofs of advertisements which were to appear in a new international wool-trade publication. All advertisements were of the same size, 8 in. × 7 in.; none had been published at the time: all had been 'O.K.'d' by the advertisers.

In consultation with Dr Wynn Jones, forty-five of such advertisements were carefully selected, and divided into groups of fifteen each. Into each of these groups an advertisement with similar distinctive features to two other advertisements was introduced. The three 'sets' therefore contained fifteen companion advertisements.

The average ranking according to merit of the three sets of fifteen advertisements by a large number of persons should give a high correlation if a common factor were present.

(*b*) *The instructions to the judges.*

Each of the three sets of advertisements was placed in a separate cover together with typed instructions which were as follows:

> Please arrange these fifteen advertisements in your order of preference from the best to the worst.
>
> *Set* 1. *Buyer*: In the order in which you think the advertisements would influence you as a buyer, disregarding the nature of the goods.
>
> *Set* 2. *Seller*: In the order in which you would yourself make use of the advertisements, in the hope of influencing people to buy your goods, disregarding the nature of the goods.
>
> *Set* 3. *Pleasant memory*: In the order in which you think they would create a pleasant memory associated with the name of the firm and the goods advertised, disregarding the nature of the goods.

(*c*) *The subjects.*

Five persons in each of the following groups were invited to judge: thirty-five persons in all:

(1) Wool merchants.
(2) Worsted spinners.
(3) Advertisers.
(4) Advertising agents.
(5) Advertisement salesmen.
(6) Printers.
(7) Commercial artists.

The average of these thirty-five judgments constituted the criterion or Advertisement Standard; and, for reasons explained later, a Wool Merchants' Standard is also used.

In addition five persons in each of the following constituted control groups.

(8) Lady secretaries.
(9) Non-trade ladies.
(10) Boys, Vth form, 16 years.
(11) Boys, Elementary, 14 years.
(12) Five trials by a lady secretary.

Ten more tests were made by the experimenter in order that changes in judgment, in any one person, on different days and times, might be observed.

(*d*) *The conditions.*

The experiments were conducted throughout under well-controlled conditions, the experimenter alone being present with the subject. All identification marks were in code on the backs of the advertisements. No conversation took place during the proceedings, nor were any results shown to the subject until the close of the tests. Previous experience has shown that it is not advisable to lay down any rules or methods for grading. Whatever method was employed a final careful examination was invariably made by the subjects.

(*e*) *Statistical methods.*

Spearman's Footrule formula is used throughout, in calculating the correlations[1]. The percentage gradings of the advertisements are determined by averaging the placings of the various group judgments, and converting these placings into a scale on which 100 per cent. is the highest and 0 per cent. the lowest. The difference between the highest and lowest figure on such a scale measures the reliability (M.R.) of the group likes and dislikes of the materials used in tests.

III. THE RESULTS AND CONCLUSIONS.

The results may, with advantage, be stated in general, rather than in detailed terms. These must necessarily be tentative and subject to further confirmation.

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[1] The numbers (n) used throughout the tests being the same (fifteen), the P.E., except where otherwise stated, is ±0·19.

(*a*) *Mass judgment.*

It might reasonably be anticipated that there would be a moderate difference between the average gradings of the thirty-five persons accustomed to dealing with advertisements and the control groups. The results are as follows, the number of persons or judgments being in brackets. ('All' includes two series of trials by the experimenter.)

Advertisement (35) and 'all' (70).

Set I	+·96
II	+·94
III	+·85
Average	+·92 ±·19

Advertisement (35) and non-trade (25).

Set I	+·88
II	+·76
III	+·60
Average	+·75 ±·19

Advertisement (35) and the experimenter (10).

	First trials	Second trials
Set I	+·81	+·66
II	+·81	+·73
III	+·50	+·31
Average	+·71 ±·19	+·57 ±·19

These figures indicate that there is a substantial agreement between the various groups. They show also, in a general way, that the fifteen styles of advertisement appeal to the natural judgments of individuals as well as to the acquired judgments gained by advertising experience[1].

It will be noted also that there is most agreement in Set I, less in Set II, with a marked drop in Set III.

(*b*) *Styles of advertisements preferred.*

The wool textile trade is divided up into a large number of specialist sections, which constitute almost separate trades, wool merchants and worsted spinners being two of the general divisions. In view of these conditions, it seemed best to take as an Advertisement Standard the opinion of those groups which contribute to the lay-out of advertisements, including the two main trade advertisers. As the wool merchants were an exceptional selection, these are treated as a Wool Merchants' Standard for comparison.

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[1] In order to compare the gradings of the advertisement and control groups (which in this case included two groups by the experimenter) the whole of the forty-five advertisements were arrayed in the order of their percentage scoring: this resulted in a correlation of +0·79±0·08.

If the Advertisement and Wool Merchants' criteria be taken as standards the following are the styles preferred, these being the average percentage gradings of the three sets of advertisements. The advertisements considered to be the best head the list.

	% grading		
Style	Advertisement	Wool merchants	
H.	(1) 90	(3) 79	In which the suggestion of the personal handling of wool is dominant.
T.	(2) 76	(4) 76	In which handling of wool is suggested through the sense of sight.
E.	(3) 71	(1) 89	Illustrations of engines.
S.	(4) 62	(5) 62	Illustrations of charts and laboratories.
C.	(5) 61	(2) 81	Coloured.
O.	(6) 52	(10) 43	Copy and display by the advertisers.
L.	(7) 46	(8) 48	Illustrations of combing and spinning machinery.
R.	(8) 44	(6) 56	Red and black lettering.
A.	(9) 42	(12) 27	Copy displayed by commercial artists.
Z.	(10) 39	(11) 39	Illustrated but crowded displays.
B.	(11) 38	(9) 44	Black grounds.
M.	(12) 38	(7) 53	Illustrations of mills.
W.	(13) 36	(13) 24	Illustrations of warehouses.
P.	(14) 29	(14) 16	Copy displayed by printers: no illustrations.
U.	(15) 26	(15) 14	Reasoned statements without illustrations.
	M.R. 64	75	

The correlation between the two gradings is + 0·83 ± 0·19. The strength of group opinion is indicated in the advertisement group at 64 per cent. and the wool merchants' group at 75 per cent.—both, judging by experience of other tests, being quite a decisive figure.

Taking the trade as a whole, I think the advertisement grading the better criterion. At the upper end of the scale the difference in the placings between the two groups is probably due to the indecision occurring in Set III, which was noted throughout the tests. The most attractive of the forty-five advertisements was designed for a keen competitor of four of the wool merchant judges, and it may be suspected that this and other advertisements of H. and T. styles were unconsciously placed a little lower on this account than E. and C. styles.

Photographs of men handling wool, suggesting the active co-operation of the senses of sight and touch with judgment appear, according to the Advertisement Standard, to be most attractive. The next most desirable are those in which there are illustrations of wool only, without men, personal handling being suggested indirectly through the sense of sight.

An examination of the different styles of lay-out shows in some measure at the bottom of the scale what it is desirable to avoid. The middle area suggests rather featureless characteristics which might be made more individual and striking.

(*c*) *Individual advertisements preferred.*

The following description of the advertisements receiving the five highest and the five lowest scores of the forty-five advertisements indicates the preferences of the thirty-five Advertisement Standard judges acquainted with the needs of the trade.

Style	Score (%)	
H.	86	This advertisement was drafted by the experimenter as a test of psychological appeal. In bold type was the wording: "The velvety touch in Merino tops is absolutely essential if you wish to produce soft handling yarns." A photograph of one of the principals of the firm 'drawing' a 'top' (combed wool) from the ball, as in judging the material, occupied the centre of the page.
T.	81	The chief feature was a photograph of a chart of fifteen standard qualities of tops made by the firm, prepared and certified by the City of Bradford Conditioning House. Much care was taken in the preparation of the card on which the fineness and length of wool fibres are displayed. The seal of the Conditioning House added to the attraction.
H.	81	"London, the world's market for wool" headed a photograph of an aisle of bales, cut open, in the show-room, with three buyers looking at and handling the raw wool in course of their estimation of quality, 'yield' (clear wool left after the removal of grease and dirt) and price.
H.	78	A bold heading of the name and address of a well-known Australian firm of wool-brokers appeared over a large photograph of "a corner of our Freemantle Showroom." This showed an immense shed with wool clearly displayed, particularly in the foreground and with a few persons in the background.
L.	72	The name and address of the firm headed a photograph of "an Improved Noble Comb." The machine was combing scoured wool, the 'tops' (combed wool) being prominently shown.
U.	31	Four-fifths of the space was closely filled with a statistical statement of wool production in one of our colonies. The heading consisted of the name of the colony and "Wool and Mohair." The only illustration was a crest of the colony.
P.	31	This merely contained name and address, "Clothing Wools," Importers and Exporters of a variety of wools and wastes, without illustration or fancy border.
Z.	27	A crowded display with black ground blocks lettered in white: "Good Packing Pays" and "Textile Goods made up for all Markets." These were surrounded by twelve lots of very small type descriptions in five languages.
L.	26	Two small photographs of spinning frames were inserted near the top of the page and below the name. The remainder of the space was filled in by small type descriptions of frames in three languages.
U.	25	An advertisement, in mixed type, with unbroken straight lines on the border and text.

(*d*) *Group differences.*

Two measures are available for comparing the variations between group judgments: (*a*) the percentage differences between the best and worst advertisements (M.R.); (*b*) the averages of ten inter-correlations of the judgments of five persons. These are as follow:

	M.R. (%)	Correlation
Trade:		
Wool merchants	75	+·41
Worsted spinners	81	+·34
Advertisers	80	+·30
Advertising agents	70	+·27
Advertising salesmen ...	68	+·25
Printers	76	+·39
Commercial artists	77	+·59
Non-trade:		
Lady secretaries	71	+·37
Non-trade ladies	65	+·23
Boys, Vth form	52	+·36
Boys, Elementary (14 years)	73	+·48

The percentage figures (M.R.) show that, with the exception of a group of Vth form boys at 52 per cent., there are well-developed likes and dislikes in groups on all the three sets.

The correlations are low and show that, between the strong likes and dislikes, there is considerable variation in the middle area. It is significant that commercial artists have the highest correlation at + 0·59, though (as will be seen later) they are in agreement with Advertisement Standard at + 0·44, the highest being + 0·54. This is no doubt due to the fact that the artist has a strong tendency to select by artistic merit only, ignoring other factors.

(*e*) *Group correlations with Advertisement and Wool Merchants' Standards.*

The averages of five judgments of each of the three sets of advertisements compare with the (1) Advertisement Standard; (2) Wool Merchants' Standard as follows:

	Advertisement Standard	Wool Merchants' Standard
Experimenter: First series	+·72	+·60
Second series	+·56	+·37
Wool merchants	+·54	—
Advertisement salesmen ...	+·48	+·81
Advertising agents	+·48	+·56
Advertisers	+·45	+·51
Commercial artists	+·44	+·16
Worsted spinners	+·43	+·76
Printers	+·42	+·40
Lady secretary—1st series	+·41	+·38
Lady secretaries	+·40	+·61
Boys, Elementary (14 years)	+·34	+·62
„ Vth form	+·35	+·60
Non-trade ladies	+·27	+·35

It would be expected that the repeated judgments of any one person would be more constant than the same number given by different people. But it would not be anticipated that a considerable variation might exist between those of, say, the experimenter, first series at + 0·72 and second series at + 0·56.

(*f*) *Common factors in judgment.*

When the methods of the investigation were planned it was anticipated that, even though the instructions given to judges were varied in each of the three sets, there would be a high correlation between them. The results of the averages of the three inter-correlations between Sets I and II, I and III, and II and III are as follows. The figures in brackets show the ranking of each group.

Advertisement Standard ...		+·61
Trade:		
Wool merchants (Standard)	(4)	+·54
Worsted spinners	(9)	+·44
Advertising agents	(11)	+·33
Advertisers	(10)	+·40
Advertising salesmen ...	(5)	+·50
Printers	(1)	+·75
Commercial artists	(2)	+·62
Non-trade:		
Lady secretaries	(8)	+·45
Non-trade ladies	(7)	+·46
Boys, Vth form	(6)	+·48
,, Elementary (14 years)	(3)	+·58
Experimenter: First series ...	—	+·59
Second series	—	+·71
Lady secretary	—	+·53

Printers and commercial artists show most uniformity, due to specialised experience. Advertisers and advertising agents, on the other hand, give consideration to a great many factors which result in a variety of opinion and independence of thought. It would be expected that a higher correlation might arise in the case of the experimenter. The fact is, however, that whilst he shows well-established uniformity of preference, there is still a margin left for likes and dislikes of particular displays within that margin from time to time.

(*g*) *Repeated judgments.*

Repetition of tests by the same person affords a good indication of the reliability of his judgment and also shows his measured opinion as compared with that of others, *e.g.* Advertisement or Wool Merchants' Standard.

Two series of experiments, with an interval of six months, were undertaken in order to trace variations at different periods. There was an interval of from a day to a week between each test, so that recall of past judgments should not unduly weigh in the decisions.

The following are the correlations with the Advertisement Standard for the two series:

Sets ...	I	II	III	Average
		First series.		
1st trial	·70	·78	·66	·71
2nd „	·78	·78	·50	·69
3rd „	·66	·81	·62	·69
4th „	·78	·83	·57	·73
5th „	·81	·76	·54	·80
Average	+·75	+·79	+·58	+·72
		Second series.		
1st trial	·66	·73	·36	·58
2nd „	·66	·85	·32	·61
3rd „	·54	·70	·31	·52
4th „	·57	·83	·32	·57
5th „	·60	·81	·23	·55
Average	+·61	+·74	+·31	+·56

It will be noted that there is less agreement in the second than in the first series. The day-to-week differences in judgment are not marked. There is, however, considerable variation between the final averages of the three sets. The highest records of Set II indicate that the experimenter agrees strongly with the Advertisement Standard, but there is a much lower agreement on Set III.

If the Advertisement Standard be disregarded and only the uniformity of the various judgments be considered, then there is a striking agreement between the tests. This might be said to be the reliability of the measured opinion of the experimenter if considered in relation to his gradings of the advertisements.

This reliability may be stated as under:

	First series	Second series
Set I	+·97	+·93
II	+·94	+·93
III	+·93	+·90
Average	+·95	+·92

As each of these represents the average of ten inter-correlations, the reliability of the judgments may be said to be remarkably high. In other words, there is a very well-defined opinion on the grading of forty-five advertisements which agrees with the Advertisement Standard in the first series at + 0·72 and the second at + 0·56. There is, however, a shift of preference on the whole between experiments made six months apart.

Repeated judgments by a lady secretary, compared with the Advertisement Standard, are as follow:

Sets ...	I	II	III	Average
1st trial	·46	·60	·08	·38
2nd „	·46	·39	·31	·39
3rd „	·54	·54	·36	·48
4th „	·57	·46	·35	·46
5th „	·54	·46	·08	·36
Average	+·51	+·49	+·24	+·41

This shows a low correlation with the Advertisement Standard: Set III being again the weakest. There is not much day-to-week change.

The inter-correlation between each of the five judgments on the three sets is as follows:

Set I	+·81
II	+·88
III	+·85
Average	+·85

The judgments are uniform, but her measured opinion has little in common with the Advertisement Standard.

IV. GENERAL OBSERVATIONS ON COMMERCIAL TESTS.

(*a*) *Fundamental factors in sound judgment.*

The wool textile trade offers an ideal field for the investigation of 'judgment' in industry, because its raw material has approximately 2000 classes, the factors involved in estimating quality and value being numerous, complex, irregular and fluctuating. Tests of any kind never cover the whole of the existing facts: the judgment of experienced men, however, must attempt to do so. It is, therefore, worth while to attempt an analysis of essential features in judgment so that all those who are practically interested should know with what factors they have to operate.

(*b*) *Key factors in the judgment of wool products.*

The 'key' factors in the judgment of wool products are the criteria of fineness, softness and shortness of wool fibres. The judgments of intelligent boys in the Vth and VIth forms in standard qualities of wool-tops show that, on the average of five trials on each of these three 'key' factors, there is a close agreement with the judgments of men of long experience. The essential feature is that there must be the opportunity for comparison of qualities which must in themselves be capable of definition, *e.g.* qualities must not be blended to make mixed or complex forms of material.

(*c*) *Preference factors.*

Preferential requirements to meet either a special price, market, machinery, or individual taste within a fashion are essentially associated with the judgments of the most experienced men. Specialists in judgment industries must be able to discriminate between samples which are variations of approximately the same quality. In the case of wool this is determined (in addition to the key factors of fineness, softness and shortness) by soundness, uniformity, colour and lustre, crimpiness, character, spinning property and other items. This demands knowledge and experience, and for the lack of it no amount of theory can compensate. Memory standards are established against which single samples may be compared, the most minute differences being detected without direct comparison. Intensive experience creates an unconscious bias in favour of materials usually handled. This bias is a common feature in wool investigation.

(*d*) *Combination of factors.*

In buying and selling (which includes advertising) any or all of the numerous factors involved in sound judgment *may* be present. There is always an unknown element in competition, one seller not knowing the conditions or prices of other sellers: the buyer rarely discloses his hand. This tends to develop a type of imagination demanding estimation, speculation and some risk.

The more readily an individual can combine the various factors involved, the better will be his judgment.

The balance of thirty-five specialist judgments appears to give a grading of all-round value, relatively free from the bias of group or person. The repeated judgments of the experimenter, which are fairly constant with themselves, point to the possibility of one person being able to cover the essential points, free from bias, if he is sufficiently acquainted with the factors indicated above.

(*Manuscript received* 11 *March*, 1929.)

VIERORDT'S LAW.

By K. C. MUKHERJEE.

(*From the Psychological Laboratory of the University of Dacca.*)

According to Vierordt the space limen of a point in a limb varies inversely to its distance from the axis of its rotation. He laid it down as a general rule on the basis of his experiments in which he observed continuous decrease of space limens from the shoulder to the tips of the fingers of his subjects.

Spatial discrimination is no doubt maximal at the tip of the finger, but certain variations from the general rule appear to occur.

The palmary side of the right-hand limb was chosen for exploration. A chalk line from the shoulder to the tip of the middle finger was drawn. The middle finger, the palm, the forearm and the arm were each divided longitudinally into three equal parts, *A*, *B* and *C*. Each of the *A*, *B* and *C* parts of the middle finger and the palm was marked into small areas *a*, *b* and *c*, and each of the *A*, *B*, *C* parts of the forearm and the arm was marked into small areas *a*, *b*, *c*, *d*, *e*, *f*. Thus we obtained a series of points *a*, *b*, *c*; *a*, *b*, *c*;. . . *a*, *b*, *c*, *d*, *e*, *f*; *a*, *b*, *c*, *d*, *e*, *f*, along the line drawn from the tip of the finger to the top of the arm.

All necessary materials such as adjustable screen, felt, Spearman's aesthesiometer, etc., were used and adequate precaution taken in the experiment, which lasted several months.

Observations of the space limens of different points of the middle finger show that the tip of the finger yields to the smallest space limen in the series. This is in agreement with the law, but certain irregularities are found when the space limens of the points adjacent to joints are compared to those of the other points lying more distant from them. The points adjacent to joints are more delicately discriminative than the points in the middle part of the region. In the *A* part of the middle finger the space limen of *b* is greater than that of *a* or of *c* in five subjects, greater than *a* but less than *c* in two subjects, and greater than *a* but equal to *c* in two subjects. Even in the two cases where the liminal value of *b* is less than *c* the difference slightly varies between 0·1 and 0·2 mm., whereas in five cases the liminal value of *b* is clearly greater than that of c, and in the remaining two cases they are equal to each other. Thus the liminal value of b, when compared with that of c, does not conform to the Vierordt's law. In the B part there is not much deviation from what is found in the A part of the middle finger, but in its C part the space limens are in line with the law. In the palm of the hand the mean limen of the B part is greater than that of A or of C parts except in the cases of two subjects. Thus the sensitivity of the middle part of the palm is less than that of the part lying longitudinally on either side of it. The recorded limens of the forearm show a markedly small sensitivity of its middle parts though a slight divergence occurs in the case of two subjects. This relative deficiency of sensitivity of the middle parts, as compared with that of the upper and lower parts terminating in the joints on both sides, is very convincingly found in the case of the arm in which the upper part towards the shoulder proves most sensitive. Thus a general survey of the records shows that the tip of the middle finger is most sensitive here, but the other extreme part at the shoulder which should, according to the Vierordt's law, be the dullest, displays on the contrary a fairly delicate sensitivity. Over the remaining parts of the arm the general trend of sensitivity is in agreement with the law except that the areas adjacent to joints are more sensitive than the areas lying between two joints.

This result agrees with general considerations relating to the positive correlation of the mobility of points with delicacy of spatial discrimination, for the areas adjacent to joints more capable of the former are more free to exploratory movement than those which lie farther away.

It is not easy to say what are the physiological peculiarities corresponding to this difference in the keenness of sensitivity in different areas along the arm. Apparently psychological factors such as exercise, and attention, and the general mobility of the parts concerned which no doubt contribute to the development of tactile discrimination do not give a complete explanation of the facts. The highly developed power of localisation in the blind and the delicacy of tactile discrimination at the finger-tip of compositors prove that tactile perception is improved by practice. But many conditions, such as compression of the veins, the anaemic state of a raised limb, the action of cold, strychnine, etc., alter the delicacy of tactile discrimination and sometimes render it altogether blunted. Kulpe attributes the difference in spatial discrimination of different touch areas mainly to physiological peculiarities of peripheral excitation, but the characteristic nature of the peripheral peculiarities he has not made clear. Weber has also emphasised the importance of the physiological function of peripheral nerve-endings to account for the delicacy of tactile spatial perception, though his theory seems to be more a statement of fact than an explanation.

It seems likely that the very close distribution of sensory cutaneous fibres at the joints underlies increased sensitivity in the areas adjacent to them. However the results of the experiment are not disturbed, even if we fail to discover the neural basis of difference in discriminative sensitivity; for the relation of the psychological to the physiological data is such that psychology is justified in setting problems for physiology, saying to it that the function is known and the organ must be looked for.

(*Manuscript received* 2 *May*, 1929.)

PUBLICATIONS RECENTLY RECEIVED.

Intellectual Growth in Young Children. By SUSAN ISAACS. London: George Routledge & Sons, Ltd. 1930. Pp. ix + 370. 12*s.* 6*d.* net.

Books about children concern themselves with a topic which is admittedly of wide human interest and importance, and which therefore from the nature of things should appeal to a very catholic audience. That they will in any case be read is hardly an adequate excuse for the cumbersome form they often take, but rather the contrary.

When the method is to present information in the form of selections from an original collection of records of children's conversations or behaviour, accompanying or preceding these by a critical and explanatory commentary or analysis, the most important part of the total contribution, in the case of many of them, lies in the scattered mass of recorded original material. That is the pity of it! The kind of experimental technique which it is necessary to use—observation and diligent recording of innumerable individual reactions, valuable or inconsequential—seems almost inevitably to lead to prolixity in the presentation of the material. Need it inevitably be accompanied by prolixity in the statement of general conclusions? The result so often is that appreciation of the true value of such a contribution to knowledge is restricted to the specialist or enthusiast, who alone will be prepared to devote to the collected data a not inconsiderable amount of attention and discrimination.

The present book does not altogether escape some of these criticisms, in spite of the high level attained in its frequently illuminating analyses and discussion. It is the first volume of a proposed trilogy, the object of which is to make an exhaustive study of the group behaviour of young children. It is based upon a three years' observation made on children between the ages of 2 and 10 years at the Malting House School, Cambridge. The present volume is concerned with the intellectual aspects of their mental development: a second volume is promised shortly on Social Development, and the trilogy is to be completed by a third volume of Individual Histories, which is to describe as a whole the development of certain selected children during the three years of the records. The first volume of 370 closely printed pages is therefore to be regarded only as an instalment of what the author has to say.

A preliminary chapter examines critically the value of qualitative records of children's behaviour, and outlines the methods by which records were obtained at the Malting House School. The subtle difficulties involved in an adult's attempt to observe and to interpret 'free' or unfettered childish life are clearly grasped and explained. It is frankly admitted that the presence of any adult observer in their midst will tend to affect unduly the course of events, and so militate against the chances of his ever being able to observe children's activities under really free and natural conditions. It is claimed however that the conditions which existed at the Malting House School did provide for the group of children under observation an environment almost as free as it would ever be possible to obtain.

A chapter of 23 pages describes what are called 'the conditions of observation.' A detailed description is given of the school environment, of the psychological setting of the experiment, and of the educational aims of the experimenters. Briefly, these aims were to stimulate the active enquiry of the children themselves rather than to 'teach' them, and to bring within their immediate experience every range of fact to which their interests reached out. Except in so far as their freedom was limited by the necessities of a minimal framework of routine, of bodily safety and hygiene, no restriction was placed upon the children's activities. But physical surroundings in the schoolroom and garden were so arranged as to stimulate them to active and constructive pursuits, and were modified in detail to meet changing needs as they grew older. The mass of incidents which constitute the cognitive experimental material is grouped, in the book, into three main classes according as it represents the application of knowledge already possessed to new situations, the direct increase of knowledge by experiment and observation, and the social interchange of knowledge in cognitive intercourse—this latter including the 'why's' and 'because's,' and the discussions with corrections and self-corrections. The general modes of thought of the children are concluded to be not fundamentally different from our own.

There is much criticism of Piaget's invocation of 'maturation' (in the sense of the changing 'structure' of the child's mind), to explain certain phenomena which may be rather a function of experience. The author claims that her records suggest that certain interests and certain ways of handling experience, which Piaget considers to belong to a more mature stage of growth, are actually found in much younger children under suitable conditions and became especially frequent under the particular conditions of her own group. Unlike Piaget who holds that the key to intellectual development lies in the first appearance of 'the social instincts' at about seven or eight years of age, she finds much earlier evidence of the influence of direct contact with the external physical world. Both threads must be followed in unravelling the story of development. Even in the earliest years (2–5) her children showed a strong and spontaneous interest in things and events of the physical world, and the idea of mechanical causality was soon evident in their remarks and behaviour. The extent to which these interests appear and are sustained, must be in large part a function of the environment, and of the response of influential adults, and they may easily be suppressed under the conditions of ordinary school curricula.

Evidence is adduced to show that little children do possess the ability to reason both accurately and clearly. Piaget explained the difference between the younger child and the older in this respect, as due to the fact either that the former does not reason at all, or only reasons in the form of perceptual judgment or practical manipulation. Mrs Isaacs holds that, in the younger child, verbal reasoning and the clear formulation of judgments are present, although they spring out of a practical and personal situation, are not sustained and die down again. Other striking differences between the behaviour of the Malting House School group and the children studied by Piaget are considered, and discussed in this section. The author does, however, point out that different levels of functioning can co-exist in the same child, and supplies instances which show that her children sometimes used the modes of thought that Piaget defined as ego-centric and syncretistic. But so do adults in certain circumstances. She ascribes the main dissimilarities between Piaget's data and picture of mental development and her own, partly to differences in the mental ratios of the two sets of children, and partly to the very considerable differences in their opportunity and setting, but largely to his very different method of enquiry. Piaget's 'clinical method' is criticized and qualified at length. The final conclusion is that intellectual growth certainly shows an elastic and living psychological coherence which "is most fully expressed in the continuity of development in noetic synthesis, and in the way in which the later and more highly integrated forms draw their life from the simpler and earlier." The chapter closes with a discussion of the relation between thought and phantasy, and of the bearing which progressive differences in interest may have in determining the characteristic modes of knowing and reasoning at different ages. The importance of the *direction* of concrete interests in this connection is stressed. Quotations lend support to all these points, which find further illustration in the full records supplied in the following chapter (47 pp.).

Light is thrown upon the rival educational values of nature study and zoology by the discussion and records concerned with children's biological interests (55 pp.). These suggest very strongly that, in spite of a fairly general opinion to the contrary, children between the ages of 4 and 10 are more actively and spontaneously interested in animals than in plants, with their lack of movement and sound and their generally slower and less arresting tempo. Everyday interest in the processes of life among their pets, 'looking inside' the dead ones, and following up any offered references to the human body, appeared in time actually to achieve an observational attitude towards these things and concurrently to the complicated problems of birth, growth, sex and death.

For the rest, the book contains an interesting account of four sample weeks in the life of the school (43 pp.), a classified summary of all the activities and pursuits of the children at the school (32 pp.), and finally a series of appendices. The longest and most important of these (by Nathan Isaacs, 58 pp.), is a theoretical analysis of the origin, development and function of children's 'why' questions. Contrary to Piaget, the findings support the popular belief that in these originally sporadic, casual and mild outbursts of childish interest, lies the germ of all later scientific enquiry and indeed of all systematic investigation. The attempt is made to show Piaget's view as a picture of the habitual level of functioning of the child's mind at any one time, complementary to the writer's own more general picture of the functioning of a process of progressive challenging.

While, therefore, this book will be of interest to the educationalist because it contains so provocative a picture of the school life of a group of highly intelligent little children, the psychologist will find in it a useful if rather discursive presentation of the arguments against Piaget's structural view of mental development, and in favour of the more functional or experiential view: a discussion in which the impartially minded may sometimes hear faint echoes of an even more ancient controversy—hereditary versus environmental influences—lifted here to a psychological plane.

M. D. S.

The Mental Development of the Child. By KARL BÜHLER. Translated from the fifth German edition by OSCAR OESER. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. 170. 8*s*. 6*d*. net.

The aim of this book, which is primarily intended for students and teachers, is to present in a relatively brief form a comprehensive account of modern psychological theories regarding the mental development of the child.

It begins with a consideration of the biological foundations of child psychology, about a fifth of the book being occupied with a discussion of such general considerations as the meaning of instinct, training and intellect, and the inheritance of intellectual qualities. The aims and methods of child psychology, and the physical development of a child are other topics with which this section deals, not infrequent reference being made to the importance of Köhler's work with his apes in throwing light upon various of these problems.

A whole chapter is devoted to the child in his first year of life, and includes an examination of the sources and development of language and of the nature of the infant's conscious states. The bearing of the Gestalt point of view on the last problem is particularly interesting.

In the other chapters the author considers the memory and imagination of children, summarizes the literature dealing with the topic of their drawings, outlines the evolution of childish thinking and concludes with a discussion of social behaviour.

The book is well illustrated with diagrams and photographs, and its profuse annotations will be of special value to readers who may desire to pursue further any of the points with which he deals. In his treatment of the problems under review Prof. Bühler's attitude is not that of a partisan bent on establishing the legitimacy of one theory at the expense of all others, but rather that of a synthesizer using the concept of development as a central pivot. Although, therefore, the manner of the book is a little disappointing because it is less stimulating and less lively than its subject-matter deserves, it is very well worth reading on account of its careful, painstaking examination of all the evidence at present available.

Eidetic Imagery and Typological Methods of Investigation. By E. R. JAENSCH. Translated by OSCAR OESER. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. 136. 7*s*. 6*d*. net.

This book consists mainly of two essays, the first dealing with eidetic imagery and the two types of eidetics, the basedowid (B) or hyperthyroid, and the tetanic (T) or hypothyroid; the first characterized by the resemblance of its eidetic images to true memory images, and the second by their resemblance to after-images. The second essay gives an account, which is clear but too sketchy, of the two main perceptual and temperamental types, the 'integrate' and the 'disintegrate,' and their relation to the eidetic. The integrate type, particularly common in childhood, is characterized by the close integration and the plasticity of all its mental functions in responding to any situation; the disintegrate, representing in general a higher level of mental development, responds in an analytic and 'piece-meal' fashion. It appears that the B-type of eidetic is an extreme case of the integrate type; the T-type seems to be pathological, though this is denied in the first essay. It is unfortunate for the English reader who is not acquainted with the experimental researches of Jaensch and his school that the experimental bases of these theories are not fully described, but only referred to rather perfunctorily. Thus the reader finds it difficult to comprehend the full implication of the theories or to assess their validity. The relationship of eidetic imagery, on the one hand, to the retinal phenomena of after-images, and, on the other, to the purely central phenomena of memory images, is glossed over too lightly, seeing that it forms the keystone of the theory of eidetics. Nor does there appear to be nearly enough evidence for the hypothesis that all individuals either hereditarily or during their own lifetime must pass through the integrate stage before they can reach the disintegrate. There is little doubt that the theories of Jaensch open up great possibilities for the future; but we cannot as yet determine their validity.

The Basis of Sensation: The Action of the Sense Organs. By E. D. ADRIAN, F.R.S. London: Christophers. 1928. Pp. 122. 7*s.* 6*d.* net.

This little book is an extremely clear and valuable résumé of the work on the impulses in sensory nerve fibres which has been carried out in recent years by Dr Adrian and his collaborators. A brief account of sensory nerves and of the methods of recording nerve impulses is followed by a comparison of the sensory discharges from various receptor end organs. The relations between the intensity of the stimulus, the adaptation of the sense organ, and the frequency of the resulting impulses are described, and Dr Adrian has much that is interesting to say about the functional significance of the different rates of adaptation of the different end organs. The last chapter discusses very briefly the relation between the impulses reaching the brain and the resulting sensation, and the conclusion is reached that the intensity of the sensation is probably proportional to the frequency of the impulses. But the quantitative evidence given here is not very convincing and one might wish for a much fuller treatment of this topic.

The general nature of Dr Adrian's work is, of course, well known to psychologists as well as to physiologists. This brief summary of these researches should be in the hands of all those interested in the psychology or physiology of the senses.

The Laws of Feeling. By F. PAULHAN. Translated by C. K. OGDEN. International Library of Psychology, Philosophy, and Scientific Method. London: Kegan Paul. 1930. Pp. xiv + 213. 10*s.* 6*d.* net.

Paulhan's "Les Phénomènes Affectifs, et les Lois de leur Apparition" appeared in the *Revue Philosophique* in 1884, and was enlarged and published in book form in 1887. Paulhan's work has had an important influence on the development of French psychology, but it has remained comparatively unknown in other countries. Mr Ogden has now translated it into English for the first time, and has included in this volume a translation of Paulhan's essay on "Feeling, Intelligence, and Will in General Psychology."

Paulhan's main thesis is that affective phenomena arise when a tendency is arrested, and psychical energy is thereby liberated and manifested in a relatively unadaptive way. The affective responses tend to appear suddenly, to lack co-ordination, and to monopolize the whole field of consciousness. (The relation of this theory to more recent views, such as those of Rivers and MacCurdy, need not be pointed out.) The feelings are classified in an interesting way according to the strength, persistence, complexity and degree of co-ordination of the arrested tendencies and the affective responses.

Paulhan's work is characterized by a great breadth of outlook, and a striking sense of proportion, and one can find in it many suggestions towards theories which have since been developed in much greater detail by other writers. The consideration of how far his general theory and classification of the feelings are of value would involve a discussion of recent experimental and clinical work on the emotions, which cannot be attempted here. The book is more likely to be of value to psychologists who are looking for suggestions towards a new theory than to students who are looking for an account of what has already been done.

Psychopathology: its development and its place in medicine. By BERNARD HART. 2nd edition. Cambridge University Press. 1929. Pp. 173. 8*s*. 6*d*. net.

These lectures and papers so judiciously conceived and so attractively written were first published in book form in 1927, and are now in their second edition.

Psychology in claiming to be a science has also claimed its right to a place in any consideration of the causation and treatment of disease. Dr Hart, working on the strictly scientific basis of determining the extent to which a proposed concept can be confirmed and verified by the facts, aims to examine and evaluate the degree to which medicine as an established science is justified in accepting as valid these claims on the part of psychology. And one of the most attractive things about the book is the spirit of almost judicial impartiality in which despite, or perhaps because of, the importance of the issues at stake, this aim is accomplished. It would be difficult to find in so compact a form any more lucid or more just assessment of the relative therapeutic values and limitations of such well-known psychopathological tenets or concepts as hysteria and hypnotism, suggestion and persuasion, the idea of 'purposiveness,' dissociation, Freudian analytical theories and methods, or the later contributions of Jung and of Adler.

The chapter devoted to the psychology of rumour and to a consideration of the relation it bears to the psychological forces at work in a crowd, or in an individual, contains some wise criticism of the old classical 'crowd psychology,' and helps to emphasize afresh the very real line of demarcation that there is between the ordinary emotional type of thought, 'complex' thinking, and that rare phenomenon, genuine rational thinking. The discussion should serve as an efficient astringent to all those persons who, like the reviewer, find it difficult consistently to put into practice their knowledge that, in any matter of really vital importance, man is indeed far from being a 'rational animal.'

In the present edition no material alteration has been made in the body of the book. It does however contain a new and valuable chapter on 'The Conception of Dissociation' which permits of a more detailed consideration of this topic than was possible in the original lectures. This, as Dr Hart hoped it would do, renders still more intelligible the point of view he has adopted with regard to the respective contributions to psychopathology of Janet and of Freud.

The Psychology of Insanity. By BERNARD HART. 4th edition. Cambridge University Press. 1930. Pp. v+176. 3*s*. net.

The fourth edition of this valuable and well-known book hardly needs a further introduction to psychologists. As Dr Hart explains in the preface to this edition, he has made no material alterations in the original text of 1912. He has refrained from doing this, first because the book is mainly concerned with elementary and general principles which have not been materially affected by advances in subsequent years, and secondly because the development of the subject has been so luxuriant and so complicated that to bring it properly up to date would require a new book.

In order, however, to acquaint his readers in a cursory form with the significance of the principles he originally described, to the structure and development of modern psychology and psychopathology, he has written an introduction or postscript of nineteen pages, in addition to incorporating in the body of the book a series of explanatory footnotes, together with a brief bibliography of some of the more recent psychopathological works. In the introduction he is mainly concerned with critically assessing the importance, from a psychopathological point of view, of the work of Janet, Freud, Jung and Adler.

An Introduction to Abnormal Psychology. By V. E. FISHER. New York: The Macmillan Co. 1929. Pp. x + 512. 11*s.* net.

Prof. Fisher begins his study of the abnormal by considering in some detail how very varied and how maladaptive the responses of the normal individual often are when he is faced with a difficult situation. From this it is but a step to the abnormal responses of those who are regarded as suffering from mental disorders. The various psychoneuroses and psychoses and the many theories which have been put forward to explain them are then considered. In each case it is shown that the illnesses are in reality disorders of personality, and can best be understood as such. The book ends with chapters on sleep, dreams, suggestion and the feeble-minded. There is no mention of therapy.

The volume is a useful, well-balanced introduction to the psychology of the abnormal.

Individual Diagnosis. By F. G. CROOKSHANK. Psyche Miniatures, Medical Series, No. 13. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. 89. 2*s.* 6*d.* net.

This little book forms one of a series which considers the connection between psychology and medicine, and it is of interest primarily to medical students.

On the assumption that the art of medicine consists of diagnosis, the question is raised in the first lecture, entitled 'Diagnosis and the Syndrome,' as to what diagnosis is. Dr Crookshank points out that once it has been grasped that diagnosis in terms of disease is but the adoption of one diagnostic convention—and that not necessarily the best—a great stride has been taken in the making of a physician! For the consequence of accepting Galen's alternative definition, namely that diagnosis is the thorough comprehension or understanding of all things present, is of immense importance to the outlook and methods of a medical practitioner, since it presupposes the realization of what Crookshank insists is to a doctor still a revolutionary fact, that patients rather than diseases should be the objects of his study. There follows a good deal of discussion about the nature and value of the conception of a symptom-complex or syndrome, and the essay concludes with a plea for the importance of the empirical method in medicine, and an outline of an 'individualized' diagnosis convention involving diagnosis in terms of the syndrome, together with a statement of all that can be found wrong with the patient.

The second lecture is entitled 'Organ-Jargon; or Organ States and Emotional Correlatives.' It supplies a series of interesting historical and contemporary diagnostic studies, and interprets these from the point of view of 'Human Medicine' in accordance with the diagnostic conception outlined in the preceding lecture.

Civilization and its discontents. By SIGMUND FREUD. Translated by JOAN RIVIERE. The International Psycho-Analytical Library, No. 17. London: Hogarth Press, and the Institute of Psycho-Analysis. 1930. Pp. 144. 8*s.* 6*d.* net.

Perhaps the chief value of this book lies rather in its literary artistry than in any very new contribution which it makes to the science of psychology as such. Undoubtedly it is very good reading; there is at times an almost emotional beauty in the dignified lucidity of its balanced prose, and it presents in a brief form an admirable general impression of the Freudian view-point.

After a preliminary consideration of the source of the religious spirit in man, which Freud believes to be based upon the child's initial need for a father's protection, it passes on to a discussion of the complicated forms in which this need persists in adult life, principal among these being the negative necessity to avert as best one can the pain and frustration inflicted upon the ego by the outer world. Intoxication, mysticism, neurotic illness, the sublimations of the artist or the scientist, phantasy-pleasures like the enjoyment of art and beauty, the personal delusions of the hermit or the fanatic, together with the mass delusions of organized religions, all these paths by means of which man endeavours to attain an incomplete happiness are discussed and evaluated, the significance of work and of love in this connection being stressed in a particularly vivid if not new way.

The realization of the inadequacy of our present civilization or culture to regulate, without inflicting needless individual suffering, the human relationships it alone makes possible, leads to a consideration of the nature, origins and evolution of culture or civilization. In the course of this there appear four interesting psycho-analytical surmises. The first of these is concerned with the origin of man's acquisition of power over fire; the second serves to explain woman's, as distinct from man's, inimical attitude to the claims of culture as opposed to the interests of sexual life. The third and fourth surmises are put forward in support of Freud's controversial suggestion (for which no evidence is forthcoming), that there has been a perceptible decrease in the importance of sexuality as a source of pleasurable sensations—*i.e.* as a means of fulfilling the purpose of life; this apart altogether from the unhappy effects of what he considers the grievously unjust and narrow attitude towards sexual love which, theoretically at least, has for so long prevailed in the civilization of Western Europe.

To Freud the evolution of culture comes finally to mean the struggle between Eros and the death instinct, or the destructive aspects of the tendency of individual aggression. Culture, erotically impelled to bind mankind into closely knit masses, renders the strong individual craving for aggression innocuous by introjecting or externalizing it. In this form as a super-ego or conscience it finds an outlet by exercising against itself the same harshness it would have liked to exercise against others. And out of this tension between the strict super-ego and the subordinate ego, arise those extraneous phenomena so essential to any rigid preservation of social solidarity, the sense of guilt and the need of punishment. There follows an elucidation, along familiar Freudian lines, of the complicated dual origin of this sense of guilt, of its immense cultural significance, and of its dangers.

The book concludes with a critical examination of the relationship to one another of the two analogous processes, the development of the individual and the cultural process evolving in humanity. Freud does not regard the struggle between the individual and society as necessarily irreconcilable, *i.e.* not as derived directly from the antagonism of the primal instincts of Eros and Death. At the present time, as a result of maladjustments, and through partial ignorance of the real significance of certain human needs, the defences against individual aggression seem to cause almost as much misery as the aggression itself, but however greatly restrictions and renunciations may oppress the lives of individuals to-day, he believes that they do admit of solution in the future of civilization.

Sisyphus: or the limits of Psychology. By M. JAEGER. To-day and To-morrow Series. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1929. Pp. 94. 2*s*. 6*d*. net.

The author tilts a lance with Freud and with Watson, but his attack is not conceived with much originality, and his victories are pyrrhic. Dostoievsky, Hamlet, Pirandello and others lend humanity to the theoretical discussion, and there is an interesting paragraph on the eleven-hour day in a Detroit motor-works. Salesmen and advertisers also play their part in the argument, but the ultimate appeal is to the metaphysician: can psychologist Münchausen lift himself out of the mud by his own pig-tail? This book will not help much—it should amuse.

Les Causes du Suicide. Par MAURICE HALBWACHS. Paris: Félix Alcan. 1930. Pp. viii + 520. 70 fr.

Prof. Halbwachs, known already for his most interesting study of the social framework of memory, has here produced another important contribution to social psychology. He deals with the most common forms of suicide in the modern community in relation to age, sex and occupation; gives details of the distribution of suicides in Europe and particularly in France, Germany, Italy and England; considers the relative frequencies of suicide in town and country; discusses the influence of family and religious life; compares suicide with murder; demonstrates the effect of war and of social and economic crises, and considers rather fully the predisposing conditions of a pathological order. He is very strongly of the opinion that suicide is primarily a social phenomenon and that by far the most important feature in its causation is social maladaptation. "We go," he says, "farther than Durkheim along the path which he essayed to travel. For we explain by social conditions not only the great factors which lead to suicide, but even all those special circumstances of the particular case which are not mere pretexts but genuine motives." The work is well documented, clear and interesting. It merits the very serious attention of all social psychologists.

Psychology and Religion. A Series of Broadcast Talks. By E. S. WATERHOUSE. London: Elkin Mathews & Marrot. 1930. Pp. xix + 232. 5*s*. net.

In the nature of the case this cannot be a very original contribution to the psychology of religion. But it is nevertheless a very good book indeed, having regard for the purpose behind its preparation. Dr Waterhouse has an adequate grasp both of the achievements and of the limitations of psychology within the field with which he is dealing. He has also a straightforward and attractive style of exposition. That his own interests run well beyond the psychological explanation of religious behaviour and experience to a strong belief in religious values he never seeks to disguise. But for all except the most hopelessly biased this increases the attractiveness of this very honest and readable book. Eighteen 'talks' dealing with most of the generally discussed problems of religion that are amenable to psychological treatment are included.

The Art of Study. By T. H. PEAR. London: Kegan Paul, Trench, Trubner & Co., Ltd. 1930. Pp. vi + 114. 3*s*. 6*d*. net.

Although this little book is meant for the general reader rather than for the student of psychology, the latter will gain not only much pleasure but a large number of attractive hints for his further study if he gives it some serious attention.

Prof. Pear is largely concerned with problems of learning and recall. He makes it perfectly clear that he is well acquainted with the mass of experimental work that has been done on these and related topics, and admirably selects from the available material that which is most important and relevant to his purpose. The book abounds with practical hints, and it is written with rare clarity and vivacity.

L'Activité Psychique chez les Animaux. Par L. BRETEGNIER. Paris: Vigot Frères. 1930. Pp. xii + 386. 50 fr.

The book deals chiefly with problems of instinct and intelligence in animals. After an historical study of early views concerning mind in animals, the author gives an account of modern behaviouristic methods of experiment. He then proceeds to deal with tropisms, reflexes and their interconnections, and instinct. Some of the most interesting parts of the discussion of instinct concern modes of orientation of animals in space. A special chapter is devoted to a study of social insects. A rather brief study of emotional and affective reactions in animals is followed by the main chapter of the whole book. This discusses animal intelligence. Nothing very new either in the way of fact or theory comes out of this, but a great amount of information of modern investigation and observation has been brought together. A final short chapter deals with language in animals. In general the author finds reason to maintain that animal intelligence though qualitatively much like that of man is quantitatively strikingly inferior. And in a familiar way he sets this down in the main to man's superior use of language. The book is not an original study, and it could be greatly condensed with advantage. Yet it certainly collects a vast mass of material from which a discerning reader will be able to gather much that is suggestive and enlightening.

Readings in Psychology. By R. H. WHEELER. New York: Thomas Y. Crowell Company. 1930. Pp. x + 597. $3.75.

There can be no doubt that this will prove a very valuable book in the hands of any discerning student who requires to get a fair knowledge of contemporary experimental work in psychology, or who is on the look out for research problems. It consists of a large number of selected 'readings' extracted from first-hand contributions to the various topics dealt with. These readings fall under seven groups: Social Behaviour; Intelligent Behaviour; Emotive Behaviour; Learning; Reaction and Observational Behaviour; Observational Behaviour; and the Nervous System. On the whole the readings are extremely well selected and they are all supplemented by further references. Though the plan of the book follows the editor's *Science of Psychology,* the readings may easily be made to serve the purposes of any student or teacher who may be using a different text. Both in plan and in performance Prof. Wheeler has done good service to psychology in this volume.

The Enlargement of Personality. By J. H. DENISON. London: Charles Scribner's Sons. 1930. Pp. xvii + 340. 10*s.* 6*d.* net.

"Here," say the publishers, "is explained a successfully tested method for becoming the person you would like to be, for obtaining happiness, peace, or whatever your *real* objective has been." The *recipe* is simple. We are what we think we are. What we think we are is in fact a heritage from a long-gone social history. Change our views of ourselves and we change ourselves. The book is popular and makes a great parade of anthropological knowledge. It cannot very well all be wrong: it certainly is not all right. The most striking thing about it is the very clever dust sheet provided by the publishers.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY

GENERAL MEETINGS.

October 25, 1930.	"Unseen Drama and Imagery: Experimental Observations." A paper by Miss Madeline Kerr, B.A. "Psychological Problems suggested by Radio-Drama." A paper read by Professor T. H. Pear.

Medical Section.

May 28, 1930.	"The Historical Background of the Problem of Temperament." A paper read by Miss May Smith.
June 25, 1930.	"Organ-Jargon." By Dr F. G. Crookshank.
October 22, 1930.	"Emotion from the Neurological Standpoint." A paper by T. R. Hill, M.D. (Lond.), M.R.C.P. (Lond.).

Education Section.

June 2, 1930.	"Some Observations on the Speech of a Young Child." A paper read by Mrs E. Norman, M.A.
October 20, 1930.	"An Investigation on the Use of Intelligence Tests with University Students." A paper contributed by Miss Barbara Dale, M.A., Ph.D.
November 10, 1930.	A Symposium on "The Nature of Stammering." Opened by Millais Culpin, M.D., F.R.C.S. and E. J. Boome, M.B., Ch.B., D.P.H.

Industrial Section.

May 29, 1930.	"Sickness Records." A paper read by Millais Culpin, M.D., F.R.C.S.
June 19, 1930.	"Personal Qualities in Accident Causation." A paper read by E. G. Chambers, M.A.
October 27, 1930.	"Perseveration as a means of testing Leadership." A paper read by W. J. Pinard, M.A., Ph.D.

Aesthetics Section.

July 18, 1930.	"The Nature of Prose." A paper read by Mrs Ursula Roberts (Susan Miles).